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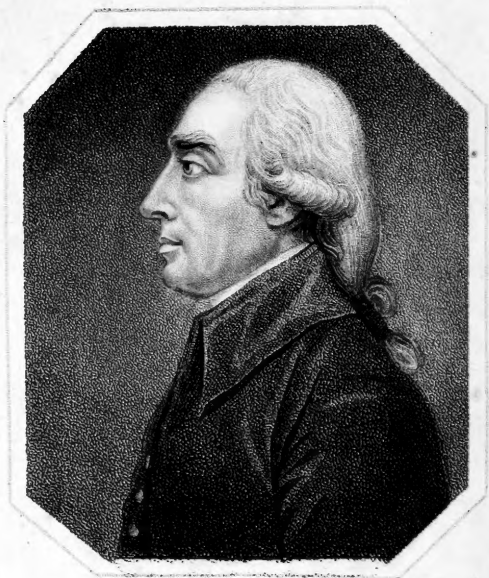




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*From Tysie.*

*Gucknitz sc.*

*Joseph Black M.D.*

*Published Aug<sup>r</sup> 1782.*

THE  
PHILOSOPHICAL MAGAZINE:

COMPREHENDING  
THE VARIOUS BRANCHES OF SCIENCE,  
THE LIBERAL AND FINE ARTS,  
AGRICULTURE, MANUFACTURES,  
AND  
COMMERCE.

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BY ALEXANDER TILLOCH,

MEMBER OF THE LONDON PHILOSOPHICAL SOCIETY, &c.

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“Nec araneorum sane textus ideo melior, quia ex se fila gignunt. Nec nostrer vilior quia ex alienis libamus ut apes.” JUST. LIPS. *Monit. Polit.* lib. i. cap. i.

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VOL. V.

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LONDON:

PRINTED BY J. DAVIS, CHANCERY-LANE,  
For ALEXANDER TILLOCH; and sold by Messrs. RICHARDSON,  
Cornhill; CADELL and DAVIES, Strand; DEERETT, Piccadilly;  
MURRAY and HIGHLEY, No. 32, Fleet-street; SYMONDS,  
Paternoster-Row; BELL, No. 148, Oxford-street;  
VERNOR and HOOD, Poultry; HARDING, No. 36,  
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THE  
PHILOSOPHICAL MAGAZINE.

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OCTOBER 1799.

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I. *Observations on Animal Electricity, and particularly that called Spontaneous.* By J. J. HEMMER\*.

UNDER Animal Electricity I understand that kind observed in animals, from whatever cause it may arise. When this electricity is excited in animals, neither by the peculiar movement of their own bodies, nor by friction, or the application of any other body, it is called Spontaneous Animal Electricity. We are taught by many instances, both ancient and modern, that men, as well as other animals, have exhibited evident signs of electricity; although the ancients, who mention these instances, did not know to what the phenomenon was to be ascribed. It may not be improper, therefore, to quote here some of the most remarkable of them.

I. We are told by Virgil, that the hair of Ascanius emitted a harmless kind of flame †.

Ecce levis summo de vertice visus Iuli  
Fundere lumen apex, tactuque innoxia molli  
Lambere flamma comas, et circum tempora pasci.  
Nos pavidi trepidare metu, crinemque flagrantem  
Excutere, et sanctos restinguere fontibus ignes.

---

\* From the *Transactions of the Electoral Academy of Sciences at Mannheim*, Vol. VI.

† *Æneid*, Lib. II. v. 582.

II. We are informed by Dionysius and other writers, that fire streamed forth from the hair of Servius Tullius, the Roman king, during sleep, when he was about seven years of age\*.

III. Pliny also speaks of light often shining around the heads of men:—*Hominum quoque capita vespertinis horis stellæ magno præfagio circumfulgent* †.

IV. We are told of a Carmelite monk, who was always attended with this phenomenon, that, as oft as he stroked his hair backwards, it emitted sparks ‡.

V. A woman at Caumont exhibited a phenomenon of the like kind, as her hair, when combed in the dark, always emitted fire §.

VI. Father J. Faber speaks of a young woman from whose head sparks of fire always fell when she combed her hair ||.

VII. Franciscus Guidus produced bright flames from his body when he rubbed his arm with his hand as he lay in bed ¶.

VIII. Ezekiel de Castro \*\*, a physician of Verona, relates the following circumstance respecting Cassandra Buri, a lady of the same place:—As often as she touched her body, even in a slight manner, with a linen cloth, it emitted sparks in great abundance, which could be perceived by every person standing near her, and which were attended with a considerable noise. Her maids were often deceived by this phenomenon, and believed that they had, through carelessness, dropped some coals between the sheets, as she always caused her bed to be warmed in winter, at which time the sparks were most abundant and strongest.

IX. Anthony Cianfi, a bookseller at Pisa, when he pulled off a narrow shirt, and a piece of cloth which he wore on his breast, emitted sparks from his back and arms, with a crack-

\* Antiq. Rom. Lib. IV.

† Hist. Nat. Lib. II. cap. xxxvii.

‡ Cardanus, Lib. VIII. de rerum variet. cap. xliii.

§ Scaliger. Exer. 174.

|| In his Palladium Chymicum.

¶ Bartholinus de luce animalium, Lugd. Bat. 1647, p. 121.

\*\* In his book *De igne lambente*.

ling noise, to the great terror of his whole family. Fortunius Licetus was a witness of this phenomenon \*.

X. "Among us," says Gefner, "where heated chambers are usual, it often happens that many persons, when they have warmed themselves at a stove, and then pull off their shirt in a cold bed-chamber, or move or shake it after it is pulled off, observe crackling flames to burst from it †."

XI. Bartholinus says, that a rope-dancer at Turin, according to the testimony of a respectable man, Cassiano a Puteo, emitted a like phenomenon of light from his body ‡.

XII. The same author says, that sparks proceed from the skins of cats, and particularly from the back; which can be clearly perceived by stroking the hair backwards even with the least pressure, and especially after they have been warmed at the fire §.

XIII. Scaliger speaks of a white Calabrian horse, which, when combed in the dark, emitted sparks of fire ||.

XIV. Ezekiel de Castro says ¶, of another horse of the same kind, that real sparks were observed when his neck was stroked upwards with the hand or a currycomb.

XV. Simpson treats of the light emitted by the bodies of animals when rubbed; and quotes instances of such phenomena on combing the hair of a woman, currying a horse, and stroking a cat with the hand \*\*.

XVI. The following circumstance respecting himself was told by Vaudania to the celebrated Beccaria:—"For ten or twelve days past, since the cold set in, I wear, between two shirts, a piece of beaver's skin. Always when I pull off my upper shirt at night, I observe that it adheres, in some degree, to the piece of skin; and when I draw my shirt from it, I see sparks which have a striking resemblance to those of electricity. Scarcely do I begin to pull off the piece of skin

\* Licetus de caussis monstrorum, Lib. II. cap. xxviii.

† Lib. de lunariis.

‡ De luce animali, p. 123.

§ Ibid. p. 189.

|| Exercitat. 174.

¶ Liber de igne lamb.

\*\* Diff. Phys. de fermentatione, 1675.

when I find that it adheres, and with still greater force, to the under-shirt. On taking it out I observe, when I hold it in the right hand, that the frill of my shirt moves up from my body towards it. If I remove the piece of skin to a greater distance, and draw it from the frill, the latter moves again towards my body. If I bring the piece of skin nearer, the shirt moves again towards it. This oscillation of my shirt between my body and the piece of skin continues alternately, till it is gradually lessened, and at length ceases."

XVII. About twelve years ago the following circumstance was communicated to our Academy, in writing, from Berdigheim:—On the 12th of February a young woman having put on an aired shift, which was exceedingly narrow and sat very close to her body, heard, on laying hold of it, a crackling noise like what oftentimes proceeds from the flame of a candle, and observed sparks to issue from all those places which she touched with her hands. Being astonished at this phenomenon, she called to another girl, who slept in the next apartment, to come to her assistance, as her shift, which she in the mean time pulled off, was on fire. They both now shook the shift in order to extinguish the sparks; but the more they moved it, the effects were stronger: on examining the shift, however, no traces of fire could be discovered. The first maid then put on another shift, which she took from her box; but as soon as she touched it, the same crackling noise and appearance of sparks took place. She then put on her dirty smock, which she had before pulled off, and returned to bed, on which the phenomenon disappeared. Some persons to whom the circumstance was afterwards related, were desirous to know whether the same phenomena would again appear if the maid put on another clean shift. She did so the following night; and as often as the smock was touched by her or any other person, a crackling noise was heard, and sparks were emitted every time the finger was brought near it. The spectators now desired her to put on another clean smock, which belonged to another maid: on approaching the hand a like noise was heard, and the appearance of fire was observed; but no sparks issued from her body, even though touched, when she had the shift on.

An

An experiment was then made to try whether the same phenomena would occur if another woman put on the girl's shift; but nothing of the kind was observed either by the person who had put it on, or by those who touched it; but the phenomena were repeated when it touched the maid on whom they had been first observed. Every time after this period that the same person put on a clean smock, the same effects were produced; but they disappeared after the shift had been worn some days. On the 1st of February the whole phenomenon ceased, and after that time never returned.

XVIII. M. Flad junior, member of our Society, having been in a consumptive state about eight years ago, observed, for a long time, as often as he pulled off or drew on his stockings, that sparks proceeded from his feet in abundance. Having afterwards recovered his strength in some measure, the sparks disappeared; but they returned when his strength again began to decrease, and continued till his disorder put an end to his existence.

XIX. As often as M. Hertel, chaplain to the Elector, draws his hand over his breast, which is covered with hair, abundance of sparks are seen in the dark.

XX. I was told by Count von Kagenek, that it often happened to him, that when he drew a silk handkerchief between his fingers, while standing near a stove, long luminous stripes were here and there observed.

XXI. I was told also by a lady named Von Fraise, that very often, when she rubbed her hands or arms even weakly with a linen cloth, lying in bed, abundance of sparks issued from them.

XXII. M. Von Schlemmer, of Deux-Ponts, relates of a lady named Von Koch, that she often observes, when she gets up early, that a ribbon, with which she binds up her hair during the night, adheres to her fingers when she pulls it off, and moves with velocity towards other neighbouring bodies. Having, by my desire, applied to the ribbon a piece of sealing wax, which had been rubbed, it was always repelled by it.

XXIII. I am acquainted with another lady, whose hair,  
when

when combed in the dark, always has a luminous appearance, as I have sometimes observed myself.

XXIV. I have a white horse, from the body of which, particularly in the winter-time, the comb brings forth abundance of sparks. I have likewise a white dog, from the back of which, when I draw my hand over it, in a warm room, from the tail to the head, sparks issue with a snapping noise, and which seem so troublesome to the animal, that he endeavours to run away. I have sometimes charged a jar with them.

XXV. I have often produced the same phenomenon on an ash-grey, strong, lively cat.

XXVI. We are acquainted with three kinds of fish which, when touched, give a shock almost like that occasioned by a charged jar, viz. the cramp-ray, *Raia torpedo* L.; the electrical eel, *Gymnotus electricus* L.; and the *Silurus electricus* L. The more modern philosophers have convinced themselves, by repeated experiments, that the effects of the shock given by these animals are of an electric nature, though to me it appears probable that they depend, in part, on another cause.

The animal electricity which I have announced in these numerous instances is of the coarser kind, and may be easily observed by every one; but there is another kind, of a more delicate nature, which does not spontaneously manifest itself, but must be concentrated and called forth by art. This finer electricity I find first mentioned in a letter of M. de Saussure to the editor of the *Journal de Physique* at Paris. The experiments which this celebrated philosopher made, partly on himself and partly on others, by means of Volta's electrometer and condenser, are given there only in extracts; but the consequences he deduces from them are as follows:

In order to produce this electricity, bodily motion is necessary. The electricity thence produced arises from the friction of the body against the clothes; for as often as he made the experiment naked, he found no electricity. In order that electricity may be produced by the friction of the body against the clothes, the latter must possess the natural warmth of the body; for when he had on clothes that were cold, he could never perceive the least trace of electricity: no electricity



tricity appears also when the body is in a state of perspiration. There are persons who, in this manner, never emit electricity. The electricity which shews itself in the human body is positive, and sometimes negative. The cause of this variation he was not able to discover.

[To be concluded in next Number.]

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II. *On the Method of Distilling as practised by the Natives at Chatra in Ramgur, and other Parts of India.* By ARCHIBALD KEIR, Esq.\*

THE body of the still they use is a common, large, unglazed, earthen water-jar, nearly globular, of about twenty-five inches diameter at the widest part of it, and twenty-two inches deep to the neck, which neck rises two inches more, and is eleven inches wide in the opening. Such, at least, was the size of the one I measured; which they filled about a half with fomented Māhwah flowers, that swam in the liquor to be distilled.

The jar they placed in a furnace, not the most artificial, though seemingly not ill adapted to give a great heat with but very little fuel. This they made by digging a round hole in the ground, about twenty inches wide, and full three feet deep; cutting an opening in the front, sloping down to the bottom, on the sides perpendicular, of about nine inches wide and fifteen long, reckoning from the circle where the jar was to come, to serve to throw in the wood at, and for a passage to the air. On the side too they cut another small opening of about four inches by three; the jar, when placed, forming one side of it, to serve as a chimney for the smoke to go out at. The bottom of the earth was rounded up like a cup. Having then placed the jar in this, as far as it would go down, they covered it above, all round, with clay, except at the two openings, till within about a fifth of its height; when their furnace was completed.

In this way I reckon there was a full third of the surface

\* From the *Asiatic Researches*.

of the body of the still, or jar, exposed to the flame, when the fire came to be lighted; and its bottom, not reaching to within two feet of where the fuel was, left a capacious hollow between them, whence the wood, that was short and dry, when lighted, being mostly converted into flame, and circulating on so great a surface of the still, gave a much stronger heat than could else have been produced from so very little fuel: a consideration well worth the attention of a manufacturer, in our country more especially, where firing is so dear. There indeed, and particularly as coal is used, it would be better, no doubt, to have a grate; and that the air should enter from below. As to the benefit resulting from the body of the still being of earthen-ware, I am not quite so clear in it. Yet, as lighter substances are well known to transmit heat more gradually and slowly than the more solid, such as metals, may not earthen vessels, on this account, be less apt to burn their contents, so as to communicate an empyreumatic taste and smell to the liquor that is distilled, so often, and so justly complained of with us? At any rate, in this country, where pots are made so cheap, I should think them greatly preferable, as at least much less expensive than those which the gentlemen engaged in this manufacture most commonly employ: though of this they are best able to judge.

Having thus made their furnace, and placed the body of the still in it, as above described, they then luted on, with moistened clay, to its neck, at the opening, what they here call an *adkur*; forming with it, at once, a cover for the body of the still, with a suitable perforation in it to let the vapour rise through, and the under-part of the alembick. The *adkur* was made with two earthen pans, having round holes in their middles of about four inches diameter; and, their bottoms being turned opposite the one to the other, they were cemented together with clay; forming a neck of junction thus of about three inches, with the small rising on the upper pan. The lowermost of these was more shallow, and about eleven inches wide, so as to cover exactly the opening at the neck of the jar, to which they luted it on with clay. The upper and opposite of these was about four inches deep,

and fourteen inches wide, with a ledge round its perforation in the middle, rising, as is already said, from the inner side of the neck, of about half an inch high, by which a gutter was formed to collect the condensed spirit as it fell down; and from this there was a hole in the pan to let it run off by; to which hole they occasionally luted on a small hollow bamboo, of about two feet and a half in length, to convey it to the receiver below. The upper pan had also another hole in it, of about an inch square, at near a quarter of its circumference from the one below just spoken of, that served to let off the water employed in cooling; as shall be mentioned presently.

Their *adkur* being thus fitted to the jar, they completed the alembic by taking a copper pot, such as we use in our kitchens, of about five inches deep, eight wide at the mouth, and ten at the bottom, which was rather flattish; and turning its mouth downward, over the opening in the *adkur*, luted it down on the inside of the jar with clay.

For their cooler they raised a seat, close upon, and at the back part of the furnace, about a foot higher than the bottom of the copper pot. On this they placed a two or three gallon pot, with a round hole, of about half an inch, in the side of it; and to this hole, before they lighted their fire, they luted on a short tube of a like bore; placing the pot, and directing its spout, so as that, when filled with water, it threw a constant and uniform stream of it from about a foot high, or near the centre of the bottom of the copper pot, where it was diffused, pretty completely, over its whole surface; and the water falling down into the upper part of the pan of the *adkur*, it thence was conveyed, through the square hole already mentioned, by a trough luted on to it for that purpose, to a cooling receiver a few feet from the furnace; from which they took it up again, to supply the upper pot as occasion required.

As their stock of water, however, in this sort of circulation, was much smaller than it seemingly ought to have been, being scarcely more than six or eight gallons, it too soon became hot; yet, in spite of this disadvantage, that so easily might have been remedied, and the shortness of the conducting tube, which had nothing but the common air to

cool it, there ran a stream of liquor from the still, and but very little vapour rising from it, beyond any thing I had ever seen from stills of a much larger size, fitted with a worm and cooler. In about three hours time, indeed, from their lighting of the fire, they drew off full fifteen bottles of spirit; which is more by a great deal, I believe, than could have been done in our way from a still of twice the dimensions.

The conveniences of a worm and cooler, which are no small expence either, I have myself often experienced; and if these could be avoided in so simple a way that might easily be improved, the hints that are here offered may be of some use. The thin metal head is certainly well adapted, I think, to transmit the heat to the water, which is constantly renewed; and which, if cold, as it ought to be, must absorb the fastest possible: whereas, in our way, the water being confined in a tub, that, from the nature of its porous substance, in a great degree, rather retains than lets the heats pass away, it soon accumulates in it, and becomes very hot; and, though renewed pretty often, never answers the purpose of cooling the vapour in the worm so expeditiously and effectually as is done by their more simple and less expensive apparatus. In this country, more especially, where labour and earthen-wares are so cheap, for as many rupees, and less, twenty furnaces, with stills, and every thing belonging to them, independent of the copper pots, might very well be erected, that would yield above a hundred gallons of spirits a-day; allowing each still to be worked only twice. So very cheap indeed is arrack here, to the great comfort of my miners, and of many thoughtless people beside, that for one single *peysa* (not two farthings sterling) they can get a whole *cutchā-fer* of it in the *basar*, or above a full English pint, and enough to make them completely intoxicated; objects often painful to be seen.

Of the superior excellence of metal in giving out heat from itself, and from vapour contained in it, we have a very clear proof in what is daily performed on the cylinder of the steam-engine: for, cold water being thrown on it when loaded, the contained vapour is constantly condensed; whence, on a vacuum being thus formed, and the weight of the atmosphere

sphere acting on the surface of the piston attached to the arm of the balance, it is made to descend, and to raise the other arm that is fixed to the pump; while this, being somewhat heavier, immediately sinks again, which carries up the piston, while the cylinder is again filled; and thus, by alternately cooling and filling it, is the machine kept in motion; the power exerted in raising the pump-arm being always in proportion to the diameter of the cylinder, or to the surface of the piston, which is exactly fitted to it, and on which the pressure acts.

The contrivance, too, of having the under part of the alembic, where the condensed vapour is collected, or upper part of what they call the *adkur*, of earthen-ware, of so great a thickness, and of course at so great a distance from the heat in the body of the still, is well imagined to keep the spirits the coolest possible, when collected, and running off.

By thus cooling and condensing the vapour, likewise, so suddenly as it rises, there is, in a great measure, a constant vacuum made, or as much as possible can be; but, that both steam rises faster, and that water boils with much less heat when the pressure is taken away from its surface, is an axiom in chemistry too well known to need any illustration; it boiling in vacuum when the heat is only ninety or ninety-five by Fahrenheit's thermometer; whereas in the open air, under the pressure of the atmosphere, it requires no less than that of two hundred and twelve, ere it can be brought to the boiling point.

I must further observe, that the superior excellence of condensing the vapour so effectually and speedily in the alembic to our method of doing it in a worm and cooler, is greatly on the side of the former; both from the reasons I have already adduced, and because of the small stream of vapour that can be only forced into the worm, where it is condensed gradually as it descends; but, above all, from the nature of vapour itself, with respect to the heat contained in it, which of late has been proved, by the very ingenious Doctor Black, to be greater by far than, before his discoveries, was imagined. For vapour he has shewn to be in the state of a new fluid, where water is dissolved by heat; with the assistance,

perhaps, if I may be allowed a conjecture, of the air which it contains; and all fluids, as he has clearly demonstrated, on their becoming such, absorb a certain quantity of heat, which becomes what he very properly calls latent heat; it being heat not appearing either to the senses or to the thermometer while they remain in that liquid state, but showing itself immediately by its effects on whatever is near it; upon their changing their form from fluid to solid, as on water becoming ice, or metals fixing, and the like. In the solution of salts, also, there is an absorption of heat, as we daily experience in the cooling of our liquors by dissolving saltpetre in water; and this he has found to be the case with water itself, and other fluids, when passing into a state of vapour by boiling. From the most accurate and judicious experiments, indeed, he infers, and with the greatest appearance of truth, that the heat thus concealed in vapour raised by boiling, from any given bulk of water, would be fully sufficient, if collected in a piece of iron of the like size, to make it perfectly red hot. What then must be the effect of so much heat communicated in our way of distilling to the worm, and to the water in the tub, will be sufficiently evident, from what has been said, to prove, I think, that we have hitherto employed a worse and more defective method than we might have done with respect to cooling at least, both in the making of spirits, and in other distillations of the like kind, where a similar mode is adopted.

The poor ignorant Indian, indeed, while he with wonder surveys the vast apparatus of European distillers, in their immense large stills, worms, tubs, and expensive furnaces, and finds that spirits thus made by them are more valued, and sell much dearer than his own, may very naturally conclude, and will have his competitors join with him in opinion; that this must alone surely be owing to their better and more judicious manner of distilling with all those ingenious and expensive contrivances, which he can no ways emulate: but in this, it would appear, they are both equally mistaken; imputing the effects, which need not be controverted, perhaps, to a cause from which they by no means proceed; the superiority of their spirits not at all arising from  
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the superior excellence of these stills and furnaces, nor from their better mode of conducting the distillation in any respect; but chiefly rather from their greater skill and care in the right choice and proper management of the materials they employ in fermentation; and, above all, as I apprehend, from the vast convenience they have in casks, by which, and from their abilities in point of stock, they are enabled, and do in fact, in general, keep their spirits for a certain time, whence they are mellowed, and improved surprisingly both in taste and salubrity.

All I need further add with respect to distillation, and on the superior advantages in the mode of conducting it here, to that we have been in use to employ, for the raising of spirits, simple waters, and the like, is only to observe, I have no sort of doubt but that the intelligent chemical operators at home, if ever they should get a hint of it, will make no manner of scruple to use it also, and to improve upon it greatly by a few ingenious contrivances, which their knowledge and experience will so easily suggest. The principles on which it seems founded, indeed, especially with regard to their way of cooling, are so striking and just, that in many other distillations besides those of spirits and waters, they may be employed, I apprehend, with very great profit and advantage. I shall now, however, confine myself to mention only the benefit that may result from a like process in the raising of the finer aromatics, while the heat contrived, as in our way, besides impeding the distillation, must, from its long action on such subtile bodies, probably injure them greatly in the essential quality on which their excellence depends; and upon this very account I am apt to imagine that the greater quantity obtained, and the superior quality of the oil of roses made in this country, to that made from roses with us, is owing chiefly, if not entirely, to their better and more judicious manner of extracting it here. For with us, the still, being made of metal, may, in the first instance, impart too great and too sudden a degree of heat; and next, the oil continuing so long in the vapour, and that much compressed, may, in so delicate a subject, not only entirely almost unite it with the water, so as to render the separation

tion impracticable, but may at the same-time alter its essence so completely, as that it can no longer appear in the state it otherwise might have been found in, had the operation been better conducted, or in the way they do here. A very few trials, however, would much better certify this than all I can possibly say on the subject, or, in fact, than all the reasoning in the world. Therefore, as to my own particular opinion of the flavour and quality of the roses at home being equal, if not superior, to that of those in this country, I may be entirely silent; the rules and reasoning in chemistry, though serving greatly to enlarge and improve our understanding, being what of themselves can never be depended upon till confirmed by facts and experiments; where many things often turn out very different from what, from our best and most plausible arguments, we had the greatest reason to expect. Or, if it should be found to be really true, what I have often heard asserted, by those however who had it only from others, but not of their own particular knowledge, that, in distilling their oil of roses at the places where they make it the best, they use also with their roses, sandal wood, and some other aromatics, no roses whatsoever, it is plain, could ever of themselves be made to afford a like oil, nor without such an addition as they employ. A circumstance, by the bye, that might possibly easily be certified by some one of the many ingenious correspondents of the Society who may happen to reside where it is made; and a knowledge of the real truth of it would certainly be of use.

III. *On the Method employed at Fez and Tetuan to prepare these Goat Skins called Morecco. By C. A. BROUSSONNET\*.*

THE skins, after being flayed from the animals by stripping them off entire, are immersed in water for three days: they are then exposed to the air, and, when dry, the hair is taken off, but imperfectly. They are then dipped in slacked lime, and sprinkled over with powdered lime to detach the

\* From the *Bulletin des Sciences*, No. 23.



smaller hairs; after which they are washed in running water, and rinsed with care. After remaining a night in the water, they are dried in the open air. Thirty parts of these skins are then placed in two quintals of bran, (each part consists of six skins, and the quintal is 150 pounds.) They are suffered to continue in this state, turning them every day, till they have acquired a great degree of pliability. They are afterwards washed again in running water, and trod upon with the feet; then thrown into a second bath made with white figs, about a quintal and a quarter of which are employed for thirty parts of skins. The figs render the water saponaceous. They are left in this bath four or five days, during which they are often turned; and, while immerfed in the water, they are for three days besprinkled with finely pounded rock-salt. The water is then drained off, and, being again besprinkled with salt, they are placed in a heap in a flat vessel, where they imbibe the salt completely. The water they contain is wrung out by twisting them; and they then become exceedingly pliable, and fit for receiving the colour.

If destined to be red, half a pound of cochineal and three ounces of alum are employed for ten parts of skins. They are then put into pits, where tan-beds are formed of about fifty pounds for each skin; which is turned in such a manner that the grain is inside, and that the skin may be filled with the tanning water. At the end of eight days they are turned inside out, and are still filled with the tanning water, in which they are left for six days; care being taken to stir them. They are then rinsed in running water; scraped with an iron instrument; cut up along the belly; and softened with a little oil. They are dried in the sun, and then cooled in the shade: after which they are gently besprinkled with water, and the edges are paired off with three different instruments of iron. When the red is too dark, a decoction of a plant called *razoul al achbi*, a kind of annual *mesembrianthemum*, is employed to weaken it. This liquid is applied warm, by pouring a spoonful over each skin.

If the morocco is intended to be yellow, the skins are prepared as for the red, but not salted till they are put into the fig-bath; and for five dozen of skins, twenty-five pounds of

tan only are employed. They are dyed with the pulverised bark of the pomegranate tree and alum.

The false red dye is communicated to skins with Brazil wood and alum. Instead of Brazil wood, *fouab*, a kind of *galium* or *rubia* (madder), brought in large quantities from Morocco, is often employed.

#### IV. *Recipe for destroying Caterpillars on Gooseberry Bushes\*.*

A RECEIPT for this purpose was offered to be communicated to the Society by William Henderson, of Baldrige Burn near Dunfermline, on the 6th of February 1795, for a suitable reward. The proposal was referred to a Sub-Committee, of which Dr. Monro, Professor of Anatomy in the University of Edinburgh, was chairman, who, after making trial of the receipt, gave in their report on the 1st of July 1796. The receipt for the preparation, and the manner of using it, was in the following words:—

Take one Scots pint of tobacco liquor †, which the manufacturers of tobacco generally sell for destroying bugs, and mix therewith about one ounce of alum; and when the alum is sufficiently dissolved, put this mixture into a plate, or other vessel, wide and long enough to admit of a brush, like a weaver's brush, being dipped into it; and as early in the season as you can perceive the leaves of the bushes to be in the least eaten, or the eggs upon the leaves, (which generally happens about the end of May,) and which will be found in great numbers on the veins of the leaves on their under side; you are then to take the preparation, or liquor, and after dipping the brush into it, and holding the brush

\* From *Prize Essays and Transactions of the Highland Society of Scotland*, Vol. I.

† Tobacco liquor is the superfluous moisture expressed from roll tobacco in the operation of pressing it; and is, in fact, only a strong infusion of tobacco in well or spring water, which may be made, where it cannot be purchased, by infusing any kind of tobacco in water till all the strength be extracted. Perhaps the sulphat of iron (copperas), employed in dyeing the roll tobacco, contributes a little to the efficacy of the liquor: a little of it may therefore be added to the infusion. EDIT.

towards the under side of the bush, which is to be raised and supported by the hands of another person; and by drawing your hand gently over the hairs of the brush, the above liquid is sprinkled and falls in small drops on the leaves: the consequence of which is, if the eggs are there, they never come forward; and if they have already generated worms, in a minute or two after the liquor touches them, they either die, or sicken so as to fall off the bush, at least they do so upon giving it a little shake. If, upon their thus falling off, they shall not appear to be completely dead, the bush should be held up, and either a little boiling water from a watering-pan thrown on them, or a bruise given them by a spade or shovel, or the earth where they lie turned over with a hoe. This preparation does not in the least injure the bushes.

The liquor here meant is generally not in the same state it is extracted from the tobacco, but is mixed by the tobacco manufacturers with cold water, in the proportion of four or five pints of water to one of the original juice or essence. Therefore, any person who may purchase the juice itself, unmixed, must mix it with water in the above proportion, and the quantity of alum must be about an ounce for each Scots pint of the mixture.

Dr. Monro's report was in the following words:—I observed, along with Mr. Hamilton and Mr. Gordon, (two other gentlemen of the committee,) and two gardeners who were present, that such caterpillars as were wetted by the liquor Mr. Henderson employs, were killed in a very few minutes, and the experiment has been repeated by my own gardener with the same effect. I have likewise found, that it kills a kind of green fly, which is very hurtful to the leaves of plum-trees and other fruit-trees. It has been very generally known, that the smoke and the juice of tobacco were pernicious to different kinds of insects and worms; but it has not, so far as I know, been employed in Mr. Henderson's manner; and as this has the advantage of not hurting the leaves, nor the fruit, I consider it as an useful and material improvement, well entitled to a moderate premium.

(Signed) ALEX. MONRO, M.D.

V. *On the gradual Changes in Temperature and Soil which take place in different Climates, with an Enquiry into the Cause of those Changes. By the Abbé MANN.*

[Concluded from Page 347 of the last Volume.]

II. *Physical Causes of the gradual Alterations in Soil and the Temperature of the Climate.*

I HAVE no doubt that a great number of different causes may have contributed, each its part, more or less, to produce those effects which are the object of the present research. Some of them are only accidental, and have taken place in different countries at very different periods, while in others some of them have not taken place at all. Among these are the draining of lakes and morasses, the extirpating of forests, and the cultivation of land. All these circumstances, no doubt, render the temperature of climate milder. I found, however, one cause, of a kind altogether different, which appears to me general and uniform in producing the above changes. After what I have before said, and in another place, it may be readily conjectured that I here speak of an union of the two distinct principles, moisture and heat. Their mutual disengagement, and the increase of the one above the other, afford, if I am not mistaken, a key to a true theory of the earth.

All the ancient writers who speak of the countries of Europe beyond the latitude of  $50^{\circ}$  north, represent them as filled with lakes and morasses, and covered with immense forests, almost as America is at present. It is a certain fact, that the climate of North America is different from that of Europe by about ten degrees of longitude; that is, the districts of North America lying under  $40^{\circ}$  of north latitude are as cold and moist as the countries of Europe which lie in the latitude of  $50^{\circ}$ . New England lies between the 41st and 46th degrees of latitude; yet it is observed that the climate there, in regard to heat and cold, is equal to that of

\* See my *Memoir on the Ancient State of Flanders* in *Mémoires de l'Acad. de Bruxelles*, Vol. I. p. 67—72.

the districts of England between the latitude of 50 and 56. It is well known that the people of America are more and more extirpating the forests, draining the marshes, and cultivating the land; and that the climate there is found to become perceptibly milder. For a thousand or two thousand years past the people in all the northern parts of Europe have been in the same manner employed in the improvement of the soil. These causes, however accidental they may be, and however much dependant on human industry, must certainly have contributed their part to render the climate milder, not only in the countries where they took place, but even in the neighbouring countries, exposed to the effects of their atmosphere.

It is almost needless to observe, that the great number of lakes and morasses, which, according to the accounts of ancient authors, existed in their time in the southern parts of Europe, must have rendered the air of these countries exceedingly cold and moist as well as unhealthful, since it lessened its elasticity, and filled it with thick vapours; which corresponds with the description they have given us of them. The countries of Europe have not for a long time been seen covered in that manner with lakes and morasses, if we except Sweden and Norway; though the places where such marshes formerly existed, both in England and on the Continent, in Gaul, Germany, and the European Sarmatia, may be still clearly observed. It is certain that human industry, in the course of ages, has in part contributed to produce this change, as a great many epochs are known when the draining of lakes and morasses was undertaken; but I have no doubt that gradual sinking of the surface of the sea, which occasions a natural and gradual efflux, that could be produced only by these means, may have contributed its part also. But, from whatever cause these changes may have proceeded, it is certain that they have contributed to lessen the moisture and cold of all the countries of Europe.

We know that in the time of Julius Cæsar, and even long after, almost all Germany and Sarmatia were covered with immense forests. The Hercynian forest was sixty days journey in length. It began in Belgic Gaul near the se,

and extended through Germany and Poland. England was proportionally less abundant in forests. Now it may be readily comprehended what extraordinary cold, what moist and unhealthful air must have prevailed in the climate of these extensive countries, as all the mountains and plains were covered with such immense woods, and as each valley almost contained a lake or a marsh; and what wonderful changes in the temperature of these lands must have been effected by the extirpation of these extensive forests, and by draining off the stagnant waters. Large woods prevent the beams of the sun from penetrating to, and warming the soil; they impede also the free diffusion of the internal heat, as the fallen leaves and branches which rot on the ground form a moist crust through which the internal and external heat can with difficulty force a passage. In the last place, they concentrate the cold and moist vapours, render them putrid, and corrupt the whole atmosphere. This has been always observed in North America, as we are assured by Dr. Williamson; and the consequences are bilious and intermittent fevers in summer and autumn, and inflammatory fevers in winter. He asserts, that the opener and drier the land becomes, the more it is remarked that these fatal diseases decrease. This must have been the case formerly in Europe under the like circumstances, and the like causes must have contributed to render its climate milder and more salubrious.

The Celts and Sarmatians, who were the first inhabitants of all the European countries lying to the north of Italy and Greece, like all the barbarous nations under different names which descended from them, and which over-ran the Roman empire in the fifth and sixth centuries, despised agriculture, and cultivated no more land than was sufficient to supply the wants of the current year. They lived chiefly either on what they caught in hunting, or the flesh of their domestic animals, of which they reared a great many; as they considered, though very unjustly, these employments as much nobler than the cultivation of land. Now it is certain that the culture of the earth, which breaks its surface, puts it in movement, keeps it in a state of continual tenderness, and makes it capable of imbibing the rays of the sun in summer, and of  
affording

affording a passage to the internal heat in winter; and by these means contributes to preserve a continual equilibrium of the principle of heat in the earth and the atmosphere. The contrary takes place in all uncultivated countries, especially when they are moist, and covered with wood.

It can no longer be doubted that the gradual draining of the stagnant water in all Celto-Scythia and European Sarmatia, with the extirpation of their large forests, and the general cultivation of the fields of these countries, must have had an influence also on the atmosphere of Greece and Italy. Those cutting north-winds which converted every thing into ice, and of which the Greeks and the Romans complain so much, have, in a great measure, ceased since the principal causes which produced them no longer exist. As long as Germany, Pannonia, Dacia, Mœsia and Thrace remained uncultivated and covered with immense forests, their atmosphere was exceedingly cold, thick and heavy, and had a continual influence on that of Italy and Greece, in which, because they were open and warm countries, the atmosphere was consequently far lighter. The exertions of this fluid to recover its equilibrium were the cause of the cutting north winds, of which the Greeks and the Romans complained so much. But after those centuries, when the whole of Celto-Scythia and Sarmatia became opener and better cultivated, their atmospheres must have come nearer to an equilibrium with that of Greece and Italy, and consequently these streams of air from the north must have decreased in the same proportion. This must have contributed to moderate the climate of Greece and of Italy, and to render it much milder than it was about 1800 or 2000 years ago; and to such a degree, that, had there been no other cause, we could no longer wonder at, or entertain any doubt of, the effects of the cold which the ancients remarked in their time, and which are not observed at present.

It is well known that the winds which traverse the immense cold regions of the Continent are always colder and more penetrating than those which blow from the sea. America extends a great way towards the north; and its remote districts, on account of the ice, snow, and continual  
fogs

fogs which prevail there, have never been explored, and therefore remain unknown. This immense country certainly extends nearer the north pole than the Continent of Europe and Asia, the boundaries of which are pretty well known. This, without doubt, may be reckoned among the causes which render North America much colder than Europe under the same degrees of latitude, as I have already remarked. Hence it happens that the immense Continent of America, the farther it extends towards the north, is more filled with lakes and marshes, which must consequently add to the severity of the winds that sweep their surface.

It will perhaps be said, that it appears to follow, from my positions, that the more the cold of winter decreases, from the above causes, the heat of summer ought to increase in the same proportion. In my opinion it can be proved by many monuments, historical as well as physical, that the sum-total of the mean summer heat is greater than it was formerly, and that it continually increases; though this takes place imperceptibly, and can be observed only after periods of considerable length, and by comparing the respective degrees of distant epochs. On the other hand, in regard to the degree of the intensity of that penetrating, concentrated, and stifling heat which is experienced even in Lapland, I must say, that this kind of heat is lessened by all those causes before enumerated which lessen the cold of winter. I by no means ascribe contrary effects to the same cause. We are taught by general experience, that the thinner, purer, and more elastic the air is, the less, in the same proportion, is the intensity of the summer heat; and, on the contrary, the thicker the atmosphere is, and the more it is filled with stagnant and concentrated vapours, the heat is more intense and the more stifling. For this reason it is always cooler on the summits of high mountains, while a stifling and insupportable heat prevails in the neighbouring plains, especially when they are surrounded by wood. This is always observed in the savannahs of America. Dr. Williamson was convinced, by experiments and observation, of the truth of my assertions in regard to North America; and he remarks, that when this extensive country becomes entirely open, when its



woods are cut down and its plains cultivated, the severity of the winter cold will not only decrease, but the stifling unhealthy heat of the summer will be moderated. The quantity of the snow, ice, and moisture is already evidently lessened; and many plants, which could not be cultivated there formerly, now thrive and succeed.

I now come to the last and principal cause of these changes, which acts in a general and uniform manner in producing these effects, as all the others are merely accidental, and depend on human industry. I here mean to say, that the principle of heat, increased continually in the course of time, so as to overcome the opposite principles of moisture and cold, renders, by these means, the earth drier and fuller of stones, and consequently increases the sum of the degree of heat. Without this principle, in my opinion, we can never find sufficient grounds for the wonderful changes which have taken place in the nature of the soil of all those lands which border on the Mediterranean sea, which formed the ancient empire of Rome from Syria to India, and which at present have all become uncommonly fruitful, dry and stony, as I have already remarked. The mere neglect of agriculture could never have produced these effects, and must have been attended rather with effects of a contrary nature\*.

#### VI. *Agenda;*

\* The author here subjoins the different passages of ancient authors alluded to in the course of this paper; but as these would occupy too much room, we must content ourselves with giving only the references, which will no doubt be acceptable to our learned readers, who may be desirous of prosecuting this subject farther:—Herodotus (469 years before the birth of Christ), Lib. IV. cap. 28, 29; M. Terentius Varro (72 years before J. C.), *De re rustica*, Lib. I. cap. 7; C. Jul. Cæsar (52 years before J. C.), *De bello Gallico*, Lib. IV. cap. 1; Virgil (50 years before J. C.), *Georgic.* Lib. III. v. 349—383; *Georgic.* IV. v. 125, 135; Diodorus Siculus, (45 years before J. C.), *Biblioth. Hist.* Lib. V. cap. 25; Ovid (10 years before J. C.), *Trist.* Lib. III. *Eleg.* IV. v. 48, 49, 51; *Eleg.* X; Strabo (28 years after J. C.), *Geograph.* edit. *Basil.* 1539, Lib. II. p. 67, 68, 107, 119, Lib. VII. p. 297; Pomponius Mela (40 years after J. C.), *De situ orbis*, Lib. II. cap. 1. *De Syria Europæa*; cap. 2. *De Thracia*; Lib. III. cap. III. *De Germania*; Columella (43 years after J. C.), *De re rustica*, edit. Sæphani 1543, in præf. n. 7, 8. Lib. I. cap. 1. p. 11, 12; L. Ann. Seneca (55 years after J. C.), *De providentia*, cap. 4. edit. *Ludz. Bat.* Tom. I. p. 711; Petronius Arbiter (60 years after J. C.), *Satyræ*.

VI. *Agenda, or a Collection of Observations and Researches, the Results of which may serve as the Foundation for a Theory of the Earth.* By M. DE SAUSSURE.

[Continued from page 359 of the last Volume.]

CHAP. XXI.

*Researches to be made in regard to the Loadstone.*

1. **T**HE theory of the loadstone ought to form a part of the theory of the earth, because the phenomena which depend on it belong to the whole globe; and because Halley, and after him other philosophers, have endeavoured to explain the different phenomena of the magnet by supposing the earth to be hollow, and that it contains in its cavity one or more magnetic globes.

2. In considering the loadstone it ought first to be examined whether, in order to explain its phenomena, we ought, like Descartes, to suppose a close fluid moving in a vortex around the magnet, and entering at one of its poles and issuing at the other; or, as M. Æpinus, a discrete fluid, susceptible of rarefaction and condensation, which is rarefied in one of the poles and condensed in the other; or, lastly,

Satyr. p. 10; Pliny the elder (74 years after J. C.), Hist. Nat. edit. *Basil* 1525, fol. Lib. IV. cap. 12. p. 60; Papinius Statius (85 years after J. C.), Sylv. Lib. V. p. 83. edit. *Amst.* 1624; Tacitus (97 years after J. C.), De moribus Germanorum, cap. 2. 4. 5; Pliny the younger (99 years after J. C.), in Panegy. cap. 12; Plutarch (101 years after J. C.); De fluviis, Tom. II. p. 1156. edit. *Frankf.* 1620, p. 949; L. A. Florus (102 years after J. C.), Lib. IV. cap. 12. edit. *Elzev.* 1610, p. 440; Appianus (130 years after J. C.), Excerpta ex ejus Celticis a Valefio, p. 1220; Pausanias (174 years after J. C.), Arcad. cap. xvii. p. 634. edit. *Leipf.* 1696, fol.; Dio Cassius (238 years after J. C.), Hist. Lib. XLIX. p. 413. edit. *Hanov.* 1606, fol.; Herodian (229 years after J. C.), Hist. edit. *Oxon.* Gr. & Lat. 1699. 8. Lib. I. p. 12. Lib. VI. p. 221; Justin (250 years after J. C.), Histor. T. II. cap. ii. p. 25. edit. *Elz.* 1664; Ammianus Marcellinus (370 years after J. C.), Lib. XIX. cap. 11; Jorvandes (525 years after J. C.), De rebus Geticis, cap. liv. p. 693. edit. *Amst.* 1655; Xiphilinus (1083 years after J. C.), in Epit. Dionis Cassii, Lib. LXVIII. p. 776, and Lib. LXXI. p. 804.

as M. Prevost\*, two fluids, susceptible of being combined with each other in such a manner that one of them alone is accumulated around the north pole of a magnet, while the other is accumulated around the south pole; and that all the magnetic phenomena may be explained by the elective attractions which these fluids exercise either upon each other or on iron†.

3. It must then be examined, whether the direction of the magnetic needle, and its inclination, depend on the situation of a large magnet enclosed in the bowels of the earth, as Halley supposes; or on the action of one of these two magnetic fluids towards one of the poles, and, perhaps, of the other fluid towards the opposite pole, as M. Prevost supposes.

4. If we admit the hypothesis of a large magnet suspended within the cavity of the earth, shall we suppose, as the inventor of this hypothesis, that this magnet has four poles? Or, shall we endeavour to explain the whole, as that great geometer Euler has done, by a magnet having only two poles? Or, lastly, shall we suppose, as Mr. Churchman, an American philosopher, has lately done, that the earth contains two magnetic poles, one at the north and the other at

\* *De l'origine des forces magnétiques*, 8. *Geneve* 1788.

† The celebrated Coulomb admits also two fluids, which compose the magnetic fluid, and which exercise their action in the inverse ratio of the square of the distance: but, in the theory of phenomena connected with the action of the globe, this philosopher sets out from certain facts immediately given by observation; one of which it would be of importance to verify in different points of the globe. This fact is, that the forces which attract one of the poles of a magnetic needle freely suspended towards the north, are equal to those which attract the opposite pole towards the south. Coulomb concludes that this equality exists, because a needle, weighed two different times, before and after it was magnetised, was found exactly of the same weight.

C. Borda has found, by observations made first at Brest, Cadiz, Teneriffe, Goree on the coast of Africa, and afterwards at Brest and Guadeloupe, that the intensity of the force exercised by the globe on the magnetic needle, estimated according to the number of oscillations made by the needle in a given time, was sensibly the same in these different places. This observation in other latitudes, especially on approaching the poles, might throw some light on the theory of natural magnetism.—Note of C. HAUY.

the south, at different distances from the poles of the earth, which perform their revolutions in different times; and that, from the combined influence of these two poles, we may conclude the annual changes of the declination with so much precision, that we can deduce the longitude of any place from its latitude, and from the degree of declination which the needle experiences\*.

5. Thus, by supposing one or more magnets in the interior part of the earth, the annual changes of the declination and inclination may be explained by the rotary movements of these magnets. But in the system, which does not admit these internal magnets, it is asked, Whether the changes of declination might not depend on movements which produce the change of obliquity, precession, nutation, and perhaps other phenomena or inequalities of that kind†.

6. With regard to diurnal variations, an English philosopher, Mr. Canton, considering that it is proved by experience that heat diminishes the force of the magnet, thought that the solar rays, by heating the earth, must lessen the attractive force of the grand magnet contained in it; and he thence deduced, as will be seen hereafter, an explanation of these variations. But Mr. Canton did not reflect on what

\* Heads of Lectures by S. Priestley, *London* 1794.

† Æpinus gives another explanation independent of these movements. It may be possible, according to this philosopher, that the declination of the magnetic needle arises, in general, from the irregular figure of the nucleus of the magnetic globe, or from an unequal distribution of the fluid in its interior part; and to account for the variation of this declination in one place, in the course of time we might suppose that the figure of the nucleus, or the distribution of the fluid it contains, is itself variable. Æpinus presumes also, that the action of the iron-mines dispersed throughout the bosom of the globe, may have an influence on the variation in question; and may, perhaps, be the sole cause of it. *Tentamen theoriæ electr. et magnet.* p. 268, 271, 334.

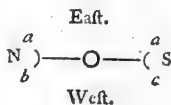
This philosopher wishes that men of science, who have an opportunity of being near a mine of loadstone, would determine, by observation, whether the masses of this mineral, before they are taken from the bowels of the earth, have their poles disposed, in regard to the poles of the world, like those of needles freely suspended: and whether, in certain masses, the poles are not in an inverse direction, of which he shews the possibility by means of consequent points. *Ibid.* p. 333.—Note of the same.

was clearly seen by M. *Æpinus*, that this magnet, if it exists, is sunk to too great a depth in the earth for the action of the solar rays, or at least the variations of that action, in the morning and evening, to be able to penetrate to it. We may, however, apply to the ferruginous minerals, dispersed in abundance over the surface of the earth, what Mr. *Canton* supposed, in regard to the grand magnet contained in its bosom; and then, if we admit that these minerals exercise any action on the magnetic needle, we cannot deny that the heat, excited by the rays of the sun, may diminish that action. From these principles it would follow, that in the morning, when the sun warms the surface of the ground situated to the east of the needle, the latter, being less strongly attracted towards that part, ought to decline towards the west; and, for a contrary reason, it must in the evening decline towards the east. But Mr. *Canton* proved, by a long series of observations, that at London, at least, this is the ordinary course of the diurnal variations.

7. But it will be proper to examine whether this explanation, even thus corrected, does not contain a paralogism; and when the attractive force of all the ferruginous particles, dispersed over the surface of the earth, to the east of the needle, is diminished equally and simultaneously, the needle ought not to remain motionless; since the diminution of the attraction exercised upon the south-pole of the needle, compensates for the diminution of that exercised upon the north-pole\*.

\* Let  $\bigcirc$  be the centre of suspension of the needle N.S., and  $a, b, c, d$ , the forces which attract the needle in opposite directions; for example, pieces of iron. The forces in  $b$  and  $d$  conspire to make the extremity N of the needle move towards the west; and the forces at  $a$  and  $c$  conspire in the like manner to make the same extremity proceed towards the east. When the needle remains at rest, there is an equilibrium, and the forces  $a + c = b + d$ . But in this supposition, if the forces of the same side,  $b$  and  $c$  for example, gradually diminish, the equilibrium will not be interrupted. For, let  $b = y + m$  and  $c = z + m$ : if the forces  $b$  and  $c$  are equally diminished by the quantity  $m$ , we shall always have  $a + z = b + y$ . The case will be the same with any augmentation, if it be equal and simultaneous on all the sides of the needle.

—Note of the Author.



I say the same thing of those situated to the west. If this reasoning be just, the needle ought not to vary by the action of the solar heat, but when this heat diminishes the magnetic force of the ferruginous parts situated to the north of the needle, more than that of the parts situated to the south, or reciprocally.

To determine this curious question, it would be necessary to make choice of two opposite coasts, and directed almost east and west from the magnetic meridian; such as the coast of Provence to the south, and that of Normandy to the north; to establish two compasses well suspended, such as those of M. Coulomb, one to the south, at Antibes for example, and the other to the north, near Cape de La Hogue; and to see whether their diurnal variations did not proceed in a contrary direction: that is to say, whether that at Antibes, having the continent to the north, and only sea to the south, would not decline, in the morning, towards the west, as that of Mr. Canton did; and whether that of La Hogue, having the continent to the south and sea to the north, would not, at the same time, decline to the east. Mr. Canton, indeed, who made these observations at London, had, to the north of his magnetic horizon, the greater part of England and all Ireland; and thus he must have had the variation west in the morning and east in the evening, as he observed it; for it is certain that the sea preserves the land, which it covers, from the action of the sun; and that thus the attraction of that land ought not to vary by the heat which emanates from that luminary.

By carefully repeating and varying these observations, in places chosen with discernment, we shall be able to decide whether the regular diurnal variation depends upon a general cause, the action of which, however, is susceptible of being suspended or disturbed by local causes; or whether we are to believe, on the contrary, as M. Van Swinden does, that the diurnal variation is not a cosmic phenomenon, or that it does not depend on a general cause inherent in the globe, and which every where acts according to the same law.

9. Is there properly any action of the magnetic fluid on the electric fluid? or is there, between these two fluids, only a resemblance of properties, or in their manner of action?

10. Is it well ascertained, as M. Van Swinden thinks, that the *aurora borealis* acts on the magnetic needle; and can any idea be formed of the mode of this action?

11. The same question in regard to the zodiacal light.

12. In general, the theory of the magnet is still so far from perfection, even in that part which depends merely on observation, that it is much to be wished that observers and observations were multiplied, especially in what concerns the inclination of the needle. In regard to the declination and its variations, M. Van Swinden has given a noble example of correctness, and of constancy in observations, and in the art of classing and comparing the results. It would be of great benefit if this example were followed in different situations and climates. It would be highly interesting, for instance, to determine, with precision, the zones of the earth where the declination is nothing, and where changes take place, and the same for the inclination.

[To be continued.]

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VII. *On the Effects produced on different Kinds of Stones by the Vapour of the Fluor Acid.* By M. KORTUM of Warsaw\*.

HAVING employed myself in engraving on glass by means of the fluor acid, and having observed that some pieces of the glass I used were more susceptible of being attacked by it than others, I resolved to try the effects of this acid on different kinds of vitreous stones. Conceiving that the figures would be corroded with more neatness, the less the siliceous earth contained in the stones was mixed with foreign matter, I exposed a very bright piece of rock-crystal, from Swisserland, at a temperature of 18° Reaum. for twenty-four hours, to the vapour of the fluor acid; and found, contrary to my expectation, that no impression was made upon it. I repeated the experiment, doubling the temperature and the time of exposure; but the stone did not lose any of its splendor. It is well known that pure rock-crystal

\* From *Voigt's Magazin für den neuesten Zustand der naturkunde*, Vol. I. Part 3.

differs from the coloured kinds by this circumstance—that by exposure to heat, the former undergoes no change, while the greater part of the latter do. It is equally well known that the action of the fluor acid on glass is increased by heat.

In order that I might begin a series of comparative experiments in this respect, I exposed, for thirty-six hours, at a temperature of  $40^{\circ}$ , a ruby, sapphire, leuco-sapphire, emerald, Oriental garnet, amethyst, chrysolite, aventurine, girasol, a Brazilian topaz burnt, a Saxon topaz raw, and an opal; but after being taken from the apparatus, they seemed as little injured as the pure transparent rock-crystal of the first experiment.

The diamond, which by its combustibility shews itself to be a peculiar genus, did not suffer the least change after four days exposure to the vapour of the fluor acid; the apparatus being placed on a common German stove.

Polished granite, being exposed to the vapour for three days on a stove as before, neither the quartz nor the mica seemed to have been attacked. The feld-spar, however, attracted my attention; being opaque and muddy, and covered with a white powder. I therefore repeated the experiment on a thin fragment of feld-spar of a reddish colour, noting its weight, which was 38 grains; and found it to be  $2\frac{1}{2}$  grains lighter. The stone had also become whitish and friable at the surface, exactly as when in a natural state of efflorescence.

The different species of flints, mixed with foreign earths, are more or less fusible. But as this difference is not exactly in proportion to the quantity of the earths mixed with them, the proper explanation of the phenomenon must be sought for in the different degrees of affinity which these kinds of earth have for caloric.

Figures traced out on the following stones through a covering of wax, after being exposed for twenty-four hours to the vapour of the fluor acid, the apparatus being placed on the stove, were all found to be etched: Chrysoliras, Hungarian opal, onyx, Persian cornelian, agate, chalcedony, green Siberian jasper, common flint. On the chrysoliras the corrosion was above half a line in depth. In those places the  
green



green colour of the stone had disappeared, and the cavities were filled with a white powder. The strokes of the figures were expressed with the greatest fineness and regularity on the opal; and the lines, in the like manner, were filled with a white powder. The onyx exhibited the contours very clearly, and the etching was pretty deep: the powder with which they were filled was likewise white. On the cornelian the figure was in part etched, and filled with white powder; in part there was only a white efflorescence, though still compact and entire. The agate and chalcedony were, on the other hand, corroded white, but very unequally: here and there cavities were formed, each of which was lined with a white compact substance. The green jasper was corroded very unequally, but almost to as great depth as the chrysopras. Some parts which remained compact, and which were only as it were effloresced, had lost their green colour and become white.

Flint. The uncovered part, of a bright brown and somewhat transparent specimen, had become totally white, but was nevertheless compact. As I had covered the stone with wax, leaving a small spot bare, without delineating any regular figure, I observed that the efflorescence had begun at the edge of the wax, and proceeded thence to the centre in such a manner that the white contour thereby produced resembled an imperfect figure of a fortification, while the inner space was only partially effloresced, and still grey, but interspersed here and there with white points. To free the stone from the wax, I washed it in spirit of wine. The white figure then gradually disappeared: in half a minute nothing of it was to be seen; and the thin fragment seemed transparent as in its native state. When it became dry, however, the opaque white figure again made its appearance. After moistening and drying it several times in succession the revival of the natural colour and opacity became always more perfect, and the white efflorescence remained visible when wet. As all these effects were produced by water and other liquids as well as by spirit of wine, as I afterwards remarked on other specimens, these phenomena of the flint had a great similarity to the change produced in the colour of the opal,

opal, and its becoming transparent in water. I found means to stop the action of the fluor acid on a cornelian and a dark brown jasper, at that stage of the process when the stone was still compact, and the colours only whitened. In water both these stones recovered their natural colour, and when dry became again white.

I exposed another opaque and almost black piece of flint, with effloresced white points, and the usual white crust, without any covering of wax, at the usual stove-heat of about  $16^{\circ}$ , to the vapour of the fluor acid for five days in my apparatus. At the end of that period it had lost almost 1-8th of its weight; for it was reduced from 103 to 91 grains, and was thoroughly white, so that the sound parts of the nucleus had entirely the appearance of the effloresced crust. Some parts were friable; and I at the same time remarked that the parts of the nucleus, already naturally effloresced, as well as the crust, were much less acted upon than the sound black parts.

It is found also, that in regard to the natural efflorescence of flints, the effloresced crust serves the sound nucleus as a cover to defend it, and, by these means, retards the total decomposition of the stone. Some, from this crust being formed of seemingly foreign materials, which is sometimes considered as lime and sometimes as clay, and which in common resembles both, have deduced too much, in regard to the formation of flints, when they explain the phenomenon by separation and successive hardening from one of these kinds of earth. To judge from the above experiments, this crust is not properly the matrix, but a result from the decomposition of the stone itself.

The efflorescence of feld-spar into a granite-like mixture is, with justice, considered as a very remarkable geological phenomenon, as it announces an endless series of the total decomposition of the present combinations. The successive degrees of the decomposition, by means of these experiments, may be exhibited with a small and very simple apparatus. Turmalin from the island of Ceylon, and Zillerthal in Tyrol, green and black columnar schorl, and olive-coloured hornblende, after twenty-four hours exposure, had experienced no change.

White

White Carara marble, in a temperature of  $20^{\circ}$ , lost, in twenty-four hours,  $\frac{1}{3^{\text{rd}}}$  of its weight; but still the shining surface of its crystallised texture was distinguishable. Weak sulphureous acid dissolved one-fourth grain out of forty-five; while, of another fragment, of 18 grains,  $1\frac{1}{2}$  grain was dissolved in the same time. Black marble suffered no loss, either in its colour or weight. Agate was not attacked.

Lamellated transparent gypsum fell into white powder on the surface, after being exposed for a few hours in the apparatus to the common heat of the stove, and the loss of its weight amounted to  $\frac{1}{2}$ . This powder was not soluble in diluted nitrous acid. From this it appears that the vapour of the fluor acid did not destroy the combination of the calcareous earth and the sulphureous acid, but only abstracted the gypsum from its water of crystallisation.

As the zeolite, among the siliceous stones, contains the greatest quantity of the water of crystallisation, as the gypsum does among the calcareous, I exposed 102 grains of striated zeolite at the stove temperature. In forty-eight hours I found its surface friable, and its weight only  $85\frac{1}{2}$  grains; consequently  $\frac{1}{8}$  less. When immersed in water, and again dried, it had increased  $2\frac{1}{2}$ . It now weighed 88 grains, but did not recover its splendour. On the tin plate, to which the stone had been fastened with wax, I observed near the latter a white powder, which had the appearance of something volatilised from the stone. I poured over the frothy cake of sparry fluor powder and sulphurous acid a little water, and, after some hours, found the inner sides of the apparatus of tin plate covered with a beautiful silky substance, of the brightness of mother-of-pearl, which was perfectly like that of the zeolite in its natural state. But as I afterwards remarked the same appearance of a splendour like that of mother-of-pearl, after exposing other stones in the same manner, it is not to be ascribed to the zeolite exclusively. On the contrary, I rather consider it as an imperfect calcination of the tin; as, by a quicker disengagement of the fluor acid, it, or a mixture of it with sulphurous acid, may acquire the property of attacking tin.

Barytes, of a fibrous texture, when exposed twenty-four

hours in the stove-heat, remained unchanged, and had sustained no loss of its weight or its splendour.

That I might not leave magnesian stone unexamined, I exposed, for forty-eight hours, at the stove-heat, a thin plate of Venetian talc weighing 124 grains. After the experiment it weighed only 81 grains, and had therefore decreased in weight more than  $\frac{1}{3}$ : it had also fallen into a soft tender powder, which floated on water, and had the appearance of magnesia. I poured water on the residuum in the apparatus, and found next day the sides incrustated with small crystalline glittering flakes, adhering in detached masses, which could not be washed off with diluted nitrous acid. Though these may have been real magnesian salt, they seem, however, to throw some light on the crystallisation of stones. Bergman had before observed the siliceous crystallisation formed in diluted fluor acid, which had stood a long time at rest over powder of silex.

Among the various hypotheses respecting the formation of granite, and stones in general, that which supposes the ancient ocean, so much spoken of by modern geologues, to have consisted of a fluid totally different from that of our sea-water, and of which the latter may be only the residuum, is not the most improbable. In my experiments, about 50 grains of the powder of sparry fluor, mixed with as much concentrated sulphurous acid as was equal to the space occupied by the powder, was put into a tin-plate box, and the latter into another of the content of about 20 square inches; to the lid of which, of strong tin-plate, I fastened, with wax, the specimens to be tried, and shut the whole so as to be air-tight. After exposing various kinds of stones for fourteen days I found the inner small vessel in part corroded, and on the exterior sides a moist, weakly-adhering salt, crystallised in a considerable quantity, which at first I considered as a mixture of iron and fluo-sulphat of lead, tin being seldom worked pure, and the solder having begun to give way. It is, however, possible that some tin may have been dissolved by the two acids combined. I dissolved some of this vitriol in distilled water, and dropped into it some spiritous tincture of galls. The liquor became of a beautiful indigo-blue colour, instead of black or purple as I expected; and a very  
light

light precipitate, of the same colour, was deposited at the bottom. A solution of muriate of barytes being dropped into the blue liquor, gave a white precipitate, like regenerated barytic spar, without changing the blue colour of the first light precipitate, which remained like a slimy substance floating on the other, nor the clear liquor: and the latter, after having stood several years, appears as deep a blue as can be produced by ammonia from a solution of copper. The blue slime was found fit for a pigment, but far inferior to Prussian blue.

On this occasion I recollected the blue colour of lapis lazuli, which Marggraf ascribes to iron, and in which Rinmann found fluor acid. This stone, after two days exposure, remained unattacked, and its colour unchanged. If, however, it is classed among the family of the zeolites, it is, at any rate, of a changed nature; for the latter are easily acted upon by the vapour of fluor acid.

According to Bergmann, siliceous earth is not soluble in pure carbonic acid gas; but it is observed in common life, in places where ammoniacal gas as well as carbonic acid gas are disengaged in abundance; such as dunghills, green-houses, prisons, cattle-stalls, and soap manufactories, &c. that the glass in the windows becomes much sooner opaque than in apartments where that is not the case. The cause of this is owing in part to a crust of foreign matter, and in part to actual corrosion. If it now be admitted that the carbonic acid gas has a considerable share in producing the efflorescence of the glass in the above-mentioned places, we may assume this effect, under certain modifications, in regard to the natural efflorescence of stones; as it is not so much a solution of the earthy bases, as a separation of the crystalline connection.

Though analogical conclusions from chemical results are of little value, they have led me to the following conjecture: As the fluor acid hitherto, at least as far as I know, has never yet been analysed, and as its radical is unknown, I am of opinion that, till something positive is learned, we may admit that fluor acid is not essentially different from carbonic acid but in the degree of oxygenation, and that they have both one radical, *viz.* carbon.

VIII. *An Easy and Cheap Method of preparing Sal Aëratus, (Carbonat of Pot-ash.)* By E. A. HOLYOKE, M. D. of Salem, Massachusetts\*.

**S**AL Aëratus, or the salt formed of vegetable alkali saturated by fixed air (carbonic acid), is, on many accounts, so useful, that a communication of an easy method of preparing it at little or no expence may be beneficial. The following may therefore be acceptable, if it be not already commonly known. I have myself prepared this salt for ten or twelve years past in this way, and it is now kept in our apothecaries shops.

Take a large wooden box †, bore eight or ten holes, half an inch in diameter, in the side of it, just below the lower edge of the cover, at nearly equal distances all round; bore also as many holes in the circular bottom of the box, close to the edge of it: then take another box of the same kind, but of a smaller diameter by half or three quarters of an inch; place this in the larger, and, to keep it steady, thrust three or four wooden wedges between the two boxes. The two boxes ‡ being thus prepared, fill the inner one with the purest salt of tartar, or clean well-calcined pearl-ashes, or any clean pure fixed vegetable alkali: put its cover on the outer box, leaving the inner one uncovered; sling this double box, thus filled, with a cord, and suspend it in a distiller's vat or cistern, while the wash is fermenting, a little above the liquor, or in an empty cistern, if it has been much used, and still retains the fixed air: let it remain in this situation for six weeks or two months, or longer if it is not wanted; let it then be taken out, and the salt, now fully saturated with the acid, be exposed to the sun and air to dry.

The salt thus prepared does neither effloresce nor deliquesce

\* From the *American Medical Repository*.

† I make use of a common cylindrical box, about nine or ten inches in diameter; and between five and six inches deep.

‡ The design of the outer box is merely to prevent any dust or dirt from getting into the salt, while the holes in it suffer the fixed air to be freely admitted.

in the open air, and, for all common purposes, is, I believe, equal to that prepared by crystallisation.

*Note.* The pearl-ashes had better be put into the box in moderate-sized lumps than in powder, that the air may have free access to it.

But if any choose to have this salt in its most perfect form, let him proceed in this manner :

Dissolve as much of the clean vegetable alkali in boiling rain or other pure water as possible ; filter the solution through paper, pour it into a jar of stone or earthen-ware, cover the vessel in such a manner as that the air may have access to it, but so as to exclude all dust or foreign matter. Let it be hung by a cord in a fermenting vat, or cistern, for a month or two, in which time a great many crystals will be formed ; from which the superfluous liquor may be poured off, and the salt dried in Hippocrates' sleeve. The superfluous liquor may be again saturated with more alkali, and again exposed to the air in the cistern, without any loss. This last is, without doubt, the most perfect mode of preparation, and I have sometimes made use of it ; but, as it is much more troublesome to make than the other, and as the other, for all medicinal purposes, is perhaps equal to this, I have for the most part employed it.

This salt is much more tolerable to the palate, and may be taken in larger doses than the naked alkali ; and as it is decomposed by vegetable acids as well as the mineral, it may be exhibited instead of the alkali in perhaps every case where the latter is proper, unless the fixed air is judged improper.

It is much superior to common alkali in forming Riverius's anti-emetic effervescent draught, as it contains a much larger proportion of fixed air (in which the principal virtue of that medicine is supposed to reside) than the mildest fixed alkali, and is at the same time much more palatable.

I commonly direct about ʒ ii, or rather more, of this salt to be dissolved in ʒ iii of fair water ; a large spoonful of this solution, added to the same quantity of good vinegar, or lemon-juice, at the instant of swallowing it, makes an agreeable dose. But the taste of this solution is so mild, that, if

the prescriber chooses, a spoonful of it may be swallowed alone first, and as much vegetable acid immediately upon it, in which case none of the gas will be lost.

When acidity abounds in the first passages, a little of this salt added to any bitter infusion, or the dry salt added to powder of columbo, or any peptic powder, is an effectual antacid.

In calculous cases this salt is recommended by writers, particularly by the celebrated Dr. Cullen in his *Materia Medica*, Vol. II. ch. 13. as being an happy expedient for conveying larger quantities of alkali into the stomach, than it can bear in its natural state.

Hitherto the common mode of preparing the salt for this purpose, I believe, has been by impregnating a solution or fixed alkali with fixed air, by means of Dr. Nooth's machine; but any one who has prepared the medicine in both ways, will readily give the most decided preference to that above described, on account both of ease and cheapness.

It is scarcely worth mentioning, that, for æconomical purposes, such as promoting fermentation in dough for bread or cakes, where pearl-ashes is commonly employed, the sal aëratum is much to be preferred, on account of the much larger quantity of fixed air eliminated in the process.

IX. *Communication from CAPEL LOFFT, Esq. respecting the late Meteor and the present Comet.*

*To the Editor of the Philosophical Magazine.*

SIR,

*Troston, near Bury, Suffolk.*

**O**BSERVING an account of the Meteor of Sunday Sept. 22, in p. 434 of your last Number, I trouble you with an account of it as observed here.

I was looking on that evening for the comet, which I expected to have seen between the Northern Crown and the constellation of Hercules. This being about half past eight, my eye consequently was then directed westward. The sky was cloudy and misty: very few stars appeared. Suddenly my attention



attention was called off from its immediate object by a most vivid reflection of light from the clouds of that part of the sky; as if full day had sprung on me instantaneously. I immediately turned round to discover whence it proceeded, and saw a most luminous body, apparently equal (or larger) to the full moon when she appears greatest, but certainly very much brighter. It was of an exceedingly splendid gold colour, and round, except to the west, where it was of a strong red, drawing off to purple, and its edge ill defined, and rather unequal. It was about  $12$  or  $15^{\circ}$  high, and almost exactly in the meridian. It seemed nearly stationary; but what little motion it had, tending to the horizon nearly at a right angle. In about three or four seconds it disappeared, as if sinking behind the clouds: I observed no sparkles, nor any luminous train left behind it, nor any explosion. It was seen by many at Bury, and was also seen at Norwich, and at Cromer on the coast north-east of Norwich. Thirty-five minutes past eight was the time I minuted of its appearance: but I had not then corrected my watch by an observation of the sun on the meridian, for many days preceding. It might be about eight or ten seconds (as I did not find it immediately, the reflection being very widely extended) between my first being struck with the reflected light, and the disappearance of the meteor. Near Norwich it was observed to throw out red sparks, or globules, as in Kent; and was noticed to be of a very white light: and the different colour of its light may be naturally referred to different states of combustion, and partly to different strata of atmosphere through which it passed.

I do not at present learn it has been seen any where much westward of the line which these several observations indicated. It seems to have been very low; and if more observations could be collected and compared, it would probably be found to have a very considerable parallax, and its altitude and magnitude might be determined, especially if observations could be had east and west of this line. But it does not seem to have been seen at London, Peterborough, Oxford, or Lincoln; or even at Cambridge, though so very little west of the places where it was seen. Yet it was scarcely possible

possible to have been not seen by any person who was out, if within the limits of the sensible horizon which circumscribed it. This seems a strong presumption that it must have been uncommonly low indeed. Very few stars being then visible, it was not easy to come to much accuracy as to its apparent path.

Meteors have abounded lately. I saw two very brilliant, but small ones, in one night, and within three minutes of time of each other; one between the Northern Crown and Bootes, and the other between the Crown and Hercules. The former appeared about the size and brightness of Venus. And on the 2d, at about twenty minutes past ten, another, which was in the field of my night-telescope with the comet: beside many smaller, usually called *shooting stars*.

The last time the weather has permitted me to see the comet, was October 4, at twenty-five minutes past ten, to a very few minutes of its setting. I thought its nucleus very discernible, and the extent of its coma rather increased. From its then appearance I should hope, if the weather favours, it will be traced down to its node; which seems likely to be in 17 or 18 of Sagittarius; and that it may still be visible for a fortnight from this time.

I remain yours sincerely,

CAPEL LOFFT.

P. S. It is worthy of inquiry and observation, whether nearly all the very large meteors have not been seen in or near the magnetic meridian, as an acute and attentive philosophical observer thinks to be the fact.

X. *Description of a Blast-Furnace for smelting Iron from the Ore, with that Part of the Blowing-Machine immediately connected with it. By Mr. DAVID MUSHET, of the Clyde Iron-Works. Communicated by the Author.*

**F**IGURE 1, (Plate I.) represents a blast-furnace, with part of the blowing-machine.

A, the regulating cylinder, eight feet diameter and eight feet

feet high.—B, the floating piston, loaded with weights proportionate to the power of the machine.—C, the valve, by which the air is passed from the pumping cylinder into the regulator: its length 26 inches, and breadth 11 inches.—D, the aperture by which the blast is forced into the furnace. Diameter of this range of pipes 18 inches. The wider these pipes can with conveniency be used, the less is the friction, and the more powerful are the effects of the blast.—E, the blowing or pumping cylinder, six feet diameter, nine feet high: travel of the piston in this cylinder from five to seven feet *per stroke*.—F, the blowing piston, and a view of one of the valves, of which there are sometimes two, and sometimes four, distributed over the surface of the piston. The area of each is proportioned to the number of valves: commonly they are 12 + 16 inches.—G, a pile of solid stone building, on which the regulating cylinder rests, and to which the flanch and tilts of the blowing cylinder are attached.—H, the safety-valve, or cock; by the simple turning of which the blast may be admitted to, or shut off from the furnace, and passed off to a collateral tube on the opposite side.—I, the tuyere, by which the blast enters the furnace. The end of the tapered pipe, which approaches the tuyere, receives small pipes of various diameters, from two to three inches, called *nose-pipes*. These are applied at pleasure, and as the strength and velocity of the blast may require.—K, the bottom of the hearth, two feet square.—L, the top of the hearth, two feet six inches square.—KL, the height of the hearth six feet six inches.—L is also the bottom of the boshes, which here terminate of the same size as the top of the hearth; only the former are round, and the latter square.—M, the top of the boshes, 12 feet diameter and eight feet of perpendicular height.—N, the top of the furnace, at which the materials are charged; commonly three feet diameter.—NN, the internal cavity of the furnace from the top of the boshes upwards, 30 feet high.—NK, total height of the internal parts of the furnace, 44½ feet.—OO, the lining. This is done in the nicest manner with fire-bricks made on purpose, 13 inches long and three inches thick.—PP, a vacancy which is left all round the outside of the first lining, three inches broad, and which is beat full of coke-dust. This space is

allowed for any expansion which might take place in consequence of the swelling of the materials by heat when descending to the bottom of the furnace.—QQ, the second lining, similar to the first.—R, a cast iron lintel, on which the bottom of the arch is supported.—RS, the rise of the arch.—ST, height of the arch; on the outside 14 feet, and 18 feet wide.—VV, the extremes of the hearth, ten feet square. This and the bosh-stones are always made from a coarse gritted freestone, whose fracture presents large rounded grains of quartz, connected by means of a cement less pure.

Figure 2 represents the foundation of the furnace, and a full view of the manner in which the false bottom is constructed.

AA, the bottom stones of the hearth. B, stratum of bedding sand. CC, passages by which the vapour, which may be generated from the damp, are passed off. DD, pillars of brick. The letters in the horizontal view, of the same figure, correspond to similar letters in the dotted elevation.

Figure 3, AA, horizontal section of the diameter of the boshes, the lining and vacancy for stuffing at M. C, view of the top of the hearth at L.

Figure 4, vertical side-section of the hearth and boshes; shewing the tym and dam-stones, and the tym and dam-plates. *a*, the tym-stone. *b*, the tym-plate, which is wedged firmly to the stone, to keep it firm in case of splitting by the great heat.—*c*, dam-stone, which occupies the whole breadth of the bottom of the hearth, excepting about six inches, which, when the furnace is at work, is filled every cast with strong sand. This stone is surmounted by an iron plate of considerable thickness, and of a peculiar shape *d*, and from this called the dam-plate. The top of the dam-stone and plate is two, three, or four inches under the level of the tuyere hole. The space betwixt the bottom of the tym and the dotted line is also rammed full of strong sand, and sometimes fire-clay. This is called the tym-stopping, and prevents any part of the blast from being unnecessarily expended.

The square of the base of this blast-furnace is 38 feet; the extreme height from the false bottom to the top of the crater is 55 feet.

XI. *Extract of a first Memoir to serve as a Natural, Chemical, and Medical History of Human Urine; with some New Facts on its Analysis and Spontaneous Alteration. By C. FOURCROY and VAUQUELIN\*.*

THERE is no animal matter which has been subjected to more examination than urine, and there is none which has furnished more discoveries to chemists. They have, however, confined themselves chiefly to an examination of phosphats, which were for a long time called Fissile Salts. Margraff, Pott, Schlosser, Haupt, and Rouelle the younger, were almost exclusively occupied with them from the time of Boerhaave to Scheele. This great attention bestowed on phosphats arose from the interest inspired by the discovery of the phosphorus of urine, and the ideas which the alchemists had propagated respecting the singular properties of these salts. What physicians have done in regard to human urine has scarcely had any relation to its nature, and has supplied too much to the ridiculous pretensions of empiricism. The discoveries of Scheele put an end to the incoherence between medical observations and the chemical labours on urine. When the acid, which forms the greater part of the urinary calculi, or the uric acid, was discovered, as well as the acid phosphat of lime, the formation of these calculi, like those of the depots of the precipitates of urine, became much easier to be comprehended than before; and it then became possible to establish between the medical observations on urine and its nature, better ascertained, that relation which ought constantly to have been the physician's guide, since by it alone he could be furnished with any exact light. The useful inferences which our colleague Berthollet has drawn from the examination of urine, and from its nature, more or less acid, in gouty affections, are at present well known.

A long series of experiments, undertaken by C. Vauquelin and myself, on animal matters in general, presented to us

\* From the *Annales de Chimie*, No. 91.

long ago a number of new facts in regard to the urine of men and animals. Our new analysis of urinary calculi induced us to resume, in greater detail, an examination of human urine, the natural source of these concretions. The result of our labour on this liquor we communicated to the Institute in the sitting of the 11th of Frimaire this year; and I shall here give a short account of it, sufficient to make it known, and to prove how much light may be thrown, by an analysis of this kind, on the physical nature of animals.

I. In this memoir the smell of human urine is first considered as a very distinct and characteristic property of this liquid. In urine well constituted, and when it issues from the bladder, it is neither the odour of ammonia, nor of an acid, nor of the violet: it is evidently aromatic, and depends entirely on a matter peculiar to urine, which makes it to be what it is, and without which it would not be urine.

II. The orange colour of urine is, no less than its odour, a property which exclusively belongs to it, and which is found in no other animal liquid. Being susceptible of many shades, and exceedingly varied degrees in its intensity, as was long ago observed by Bellini, it is indebted for this variation to the very variable proportion in the effect of the water and the colouring matter, and the latter is the same that gives to this liquid its aromatic odour. The darkest coloured urine, either naturally or by artificial evaporation, assumes all possible shades, like those remarked under different circumstances, by the addition of water only in various quantities. Thus the smelling and colouring matter of urine is very soluble in water.

III. The authors of the memoir, in treating of the acrid and strong taste of human urine, remark that this acidity is not merely that of the saline substances held in solution in this liquor, which only modify it, by giving it a saline taste. The durable acidity of urine depends also on the matter which produces its smell and colour. It varies, therefore, like these two properties, which it follows in their intensity or diminution. When physiologists ascribed the savour of urine to these salts, they did not know, or did not

pay attention to the small quantity of them in proportion to the mass of water in which they are dissolved.

IV. The component materials of urine, the number of which is considerable, re-act on each other during the process of analysing them. The salts contained in it are modified, and change their nature: but the property, above all, of becoming alcalised, or of forming, by its spontaneous alteration, ammonia and carbonic acid, (a property which is developed in a few moments in an elevated temperature, and which has made it be considered as the most *alcalescent* of all the animal humours,) is the source of the most singular changes in its nature. Instead of remaining acid, it then turns vegetable colours green; it produces an effervescence with acids when poured over it; it changes its colour; it assumes a fetid ammoniacal smell; it deposits precipitates and crystallised salts which it did not contain. This alteration begins sometimes even in the reins, and carries with it a disposition to form calculi, which it would not have formed without it. It depends entirely on the urinary matter, the cause of its odour, its colour, and its flavour.

V. It follows, from the preceding consideration, that the analysis of urine, by the means at present employed, must have given many uncertain results; and that many errors, in this respect, must have been committed. The action of fire, which so speedily and so easily alters the nature of bodies, changes both the proportions and the properties of its productions. Urine, therefore, must be examined at the moment when it issues from the body; the component parts of it must, as much as possible, be sought for without employing fire; in examining it, re-agents ought to be used, which, as in the analysis of mineral waters, may serve to ascertain, at the moment of their mixture, the matters contained in that liquid. The phosphoric, the uric, and the muriatic acids, with lime and ammonia, have already been shewn in it; but science is as yet far from being in that advanced state as to possess the number of re-agents necessary for this kind of analysis, which still requires many new researches.

VI. Our researches on the means of analysis, carried much farther

farther than any before undertaken, have been attended with no other success than to enable us to compare the phenomena they exhibited with those of evaporation. Thus the transparency of turbid urine, and the concrete flakes precipitated during the evaporation of urine, have shewn us that the caustic alkalies, and the precipitate formed by the tanning principle in the liquid, belonged to the phosphat of lime and a gelatinous animal matter. We have learned, by the same comparative process, that even a very gentle heat formed in urine ammonia, which speedily neutralised its acidity: that its colour embrowned by evaporation, and its abundant crystallisation, on cooling, after it had been brought to the consistence of syrup, depended on the concentration of the particular matter, the common source of its colour, odour, flavour, and its other characteristic properties: that the fetid garlic smell, and the crystalline form, were two of its most prominent characters: and, in the last place, that as all urine evaporated in this manner forms itself into a mass, there ought to be found in this mass the constituent matters of urine, except the portion of ammonia formed and volatilised by the evaporating heat. This lamellated crystalline mass, treated with alcohol, was almost entirely dissolved, and nothing remained but a little of the grey saline substance which the water separated from the phosphat of soda and ammonia, and a little phosphat of lime and uric acid, insoluble in the liquid, but which was insulated from the calcareous phosphat by the ley of caustic alkali. These salts, and this acid, made some milliemes only of the weight of the urine; while the matter dissolved by the alcohol formed some centiemes. The latter was composed of a little muriat of ammonia, benzoic acid, and urinary matter more abundant than all the rest. Such is the series of our analytical processes, analogous to those employed on the residues of mineral waters, and by the help of which we have been able to separate the constituent matters of human urine more exactly than had been done before.

VII. The distillation of urine, though considered as well known, presented to us several remarkable facts. In a very gentle sand-bath, fresh urine gives water very ammoniacal  
and



and crystallised carbonat of ammonia long before it is dry: the last portions of water obtained effervesce strongly with acids, and become of a rose-colour, not much susceptible of changing in the open air. It arises from the carbonat of ammonia which the liquid product contains; and urine has a singular disposition to form this salt in great abundance and with great ease, as is proved by all the means of analysis applied to it. The native acid of the urine is then saturated; flakes of animal matter are deposited, as well as earthy phosphats and the uric acid. All these phenomena continue until the predominance of the carbonat of ammonia, which is formed, becomes very manifest. They take place at a continued temperature of 60°.

VIII. We never saw, with any exactness, but three facts in regard to the putrefaction, or spontaneous and septic decomposition of urine, *viz.* the horrible fetor by which it is accompanied; the formation of a great quantity of ammonia which characterises it; and the abundance as well as easy extraction of the fusible salts or alkaline phosphat which follows. C. Hallé has given an exceeding good description of the successive alterations which urine, left to itself, experiences, but he has not followed them in regard to the nature of their materials; the object of his research was only to describe the effects in their appearances. Urine contained in a close vessel becomes of a darker colour; turns brown, and even black; emits a fetid, ammoniacal odour; deposits first a light cloud, which is gradually changed into mucous flakes more or less coloured. There are formed at its surface, or on the blackened crust by which it is covered, and on the sides of the vessel which contains it, crystals in needles or in regular prisms, or silky tufts. Urine is then ammoniacal instead of being acid. Distilled to a half, it gives a great deal of the fetid carbonat of ammonia: being carried still farther, so as to become syrupy, it furnishes acetite of ammonia: the thick part gives, by the addition of acids, a sharp and acetous odour. This residuum of urine, when putrid and evaporated, does not, by the addition of the concentrated nitric acid, present those white concrete and abundant crystals which arise in fresh urine evaporated to the same degree, and

and which belong to the urinary matter not changed. It was this matter which experienced the greatest and most singular change by putrefaction; it was the focus and subject of it; and it gave birth, above all, to the carbonat of ammonia which replaced it, and to the acetous acid, which, as the phosphoric and the uric, is found saturated by this kind of alkali. It appears, therefore, that in order to procure more of the phosphat of ammonia, it is of advantage to employ putrid urine.

IX. An attentive and careful examination of the first phenomena of the putrid decomposition of human urine, presented to us results as interesting at least as that of urine entirely decomposed. These results are connected, above all, with the formation of urinary calculi, which were one of the most important objects of our labour. We were desirous of adding to the well-known fact of the existence of calculous matter in every kind of urine, answers to the following questions of so much importance:—Why are these matters more abundant? Why does a disposition to calculus exist in some subjects and not in all, though the urine of all contains what may form them? Why are they formed sometimes speedily, and sometimes slowly? What is the cause of the six-fold variety of the calculous matters, their mixture, and interruption? Though we were far from having found a solution to all these questions, we have at any rate been able to resolve some.

The white prismatic crystals, which are deposited on the pellicles or the sides of vessels containing urine, do not shew themselves till the liquid becomes ammoniacal. They increase in quantity and bulk for six or eight days: they are prisms of six planes, with pyramids having six faces, which are readily discovered to be ammoniaco-magnesian phosphat, like that often found, under the sparry form, at the surface of white calculi. This salt does not exist, or is not formed, but when there is an excess of ammonia in the urinary liquor. This ammonia, by saturating the free phosphoric acid, separates the gelatinous matter which gives birth to a mucous precipitate, and constitutes with that acid, and by uniting itself to the phosphat of magnesia, the ammoniaco-magnesian

phosphat, which deposits itself in crystals. The uric acid is equally saturated, and presents urate of ammonia; which sometimes deposits itself in the calculous matter with the triple phosphat just mentioned. This triple phosphat separates itself in crystals of the liquor only because it is less soluble than the two insulated phosphats: This salt, therefore, which does not exist quite formed in the urine, is the produce of its ammoniacal alteration.

As ammonia and the carbonic acid go on always increasing when the uric and phosphoric acids and the phosphat of magnesia are saturated, there remains in the liquor carbonate of ammonia, which then renders syrup of violets green, and effervesces with acids. The acetous acid, which is formed at the same time, becomes saturated also with ammonia; so that the urine contains, at the same time, acetite and carbonate of ammonia. These three substances, formed almost at the same time by the decomposition of the urine; ammonia, the phosphoric and the acetous acids, are the produce of the urinary matter, which ceases to be found in that liquid when altered by putrefaction.

X. This alteration of urine does not always take place in the same manner, and its decomposition varies according to the diversified nature of that liquor. Sometimes, in one individual, when the urine commonly presents the before-mentioned phenomena, the liquid, instead of emitting an ammoniacal odour, becomes covered with a green and white mouldiness, which increases for fifteen or twenty days. Instead of containing ammonia naked, it contains the acid, and emits a smell of it. This kind of urine is less subject to alteration than the preceding. It appears to be at least as common as that which alcalises. Their difference depends evidently on the variation of their principles, and not on their nature; for they are almost always the same, except in their proportion. Thus the urinary matter, the source of the formation of ammonia, of the carbonic and acetous acids, and the cause of the alterability of urine, does not produce or experience that alteration, or that decomposition, but so far as it is mixed with a certain quantity of gelatinous matter, which serves it as a ferment. If it does not contain

enough, and if the urine, less gelatinous, is by these means more coloured, has a stronger smell, and is more charged with urinary matter, it is less susceptible of fermentation or putridity; it preserves itself much longer and with its primitive characters, and is more permanent: that, on the other hand, which is less coloured, more changeable, and more disposed to the formation of ammonia, deposits speedily mucous flakes, and gives sooner a cloud and precipitate. It appeared to us, that urine less corruptible and less gelatinous, and in some measure more urinous, if I may be allowed the expression, was the sign of good health, and the produce of complete digestion; while pale urine, more gelatinous and more decomposable, existed more particularly in weak subjects, and in cases where the digestive faculties were lessened. There is reason to believe that these two different states of urine, which exhibit it as two distinct or different liquids, will one day furnish facts of great utility to the healing art; and that a solution of tan will supply the means of distinguishing them, and of determining their relation in regard to the nature and quantity of the precipitate which it will form in these liquids, compared in different subjects, or in the same at different periods.

XI. Scheele first announced the presence of the benzoic acid in the urine of man, and particularly in that of children. Rouelle the younger had before suspected it in that of the mammeferæ, though he durst not give any opinion as to its nature. This acid is obtained by sublimation, on heating the extract of urine in close vessels. It may be separated still better by evaporating urine to the consistence of clear-syrup, and pouring on it the muriatic acid, by which it is precipitated; because that acid decomposes the benzoat of ammonia that has been formed. It is by the latter process that we have taught the method of extracting it from the urine of horses and cows, and, above all, from the water of dung-hills, in sufficient abundance to substitute it in pharmacy for that of benzoïn. In a word, this acid is the least abundant, and perhaps the most variable of the matters in urine. It appeared to us to be between  $\frac{1}{1000}$  and  $\frac{1}{10000}$ . There are some morbid circumstances under which its quantity increases

increases very sensibly, and often very speedily. We had no opportunity of determining the difference of the proportion in the urine of adults, or that of children, in which Scheele says that it is much more abundant.

XII. The analysis of urinary calculi, which first directed our attention to urine as the source of its concretions, induced us to examine whether the oxalic acid existed in this liquid. The oxalat of lime is, indeed, one of the most frequent matters of calculi; and we have found it in the proportion of a sixth in the number of urinary stones we examined. None of the means which can be employed to discover the presence of that acid, exhibited it to us in urine; while, on the other hand, the smallest quantity of the oxalic acid, which we poured into the liquid, gave us an abundant and very heavy oxalat of lime; which proved to us, that such an acid could not remain dissolved in the urine. Thus, when a mural or mulberry-formed calculous, composed of the oxalat of lime and an animal matter, by which it is agglutinated, arises in the urinary duct, its production takes place even at the moment of the formation of the oxalic acid. This acid must arise in the urine; and in that case there must be in it an unnatural and morbid foreign production. There is reason to believe that some kinds of urine, which come from the body white and turbid, are charged with this salt; and that the oxalat of lime, formed by a cause still unknown, issues in this manner without producing calculi. It may be thence seen of how much importance it is to make a chemical examination of the urine of diseased persons, and to establish a continued series of experiments on this subject in some hospital set apart for these useful researches.

The case is the same with siliceous earth as with the oxalat of lime. We have never yet found it in urine. And its existence in that liquid appears to be a rare case in pathology: of three hundred calculi, indeed, analysed with care, two only were found containing silex.

XIII. This analysis shows that several matters, hitherto unknown in urine, exist in it, *viz.* 1. Phosphat of magnesia. 2. The urat of ammonia, which is formed at the time of the decomposition of the urine. 3. Albumen and gelatinous

matter. 4. The oxalic acid, which is produced under some morbid circumstances. 5. Siliceous earth, which is found in it only very rarely. The four first substances are constant in it; the other two are only accidental, and therefore we pass them over till after the examination of calculi arising necessarily from urine, and of which the constituent matters have been dissolved in that liquid. Another new fact obtained by our labour is, that the particular matter which gives to urine its distinguishing characters, is converted, by means of the fermentation excited by the presence of the gelatinous body, into ammonia, carbonic acid, and acetic acid, and from these new products there arise in urine very remarkable changes. The analysis of putrid urine differs, therefore, very much from that of fresh urine.

It results from these researches, by which human urine has become much better known than it was before, that it contains ten principal or constant substances, characterised in the following manner;

A. Muriat of soda, which crystallises in octaedra in evaporated urine.

B. Muriat of ammonia; the natural octaedral form of which is modified into a cube by its union with the particular urinary matter like that of the preceding, and passes through the same combination from the cube to the octaedron.

C. The acid phosphat of lime, forming about the 700th part of urine, considered formerly as an earth, precipitating itself by the addition of alkalies, which take from it its excess of acid; carrying always with it a gelatinous matter which this acid held in solution, and rendering urine turbid at the moment when the ammonia is disengaged.

D. Phosphat of magnesia, decomposed by alkalies, and giving its earth mixed with the phosphat of lime which is deposited; becoming a triple salt, and separating itself in crystals by the spontaneous formation of ammonia.

E. The phosphat of soda, efflorescing in the air, and always united to the phosphat of ammonia.

F. The phosphat of ammonia, little abundant when the urine is fresh; increasing much by decomposition and the formation

tion of ammonia, and giving only phosphorus when the whole fusible salt of urine is heated with carbon.

G. The uric acid, named very improperly at first the *litbic acid*. It crystallises by the cooling of urine, and forms the red sand which that liquid deposits at the bottom of pots. It is more abundant in the urine of diseased persons. It may be dissolved exceedingly well by the caustic alkali.

H. The benzoic acid, more abundant in the urine of children; easy to be obtained from urine evaporated to a syrup, mixed to a tenth with concentrated sulphuric acid, and distilled.

I. Gelatinous matter and albumen, extremely variable in its proportion in the different kinds of urine; shewing itself in clouds in urine where ammonia is formed, in filaments in that over which alkali is poured, in flakes by the evaporation of urine; precipitating itself by the tanning principle, which serves to determine the proportion; occasioning a speedy putrefaction of urine, which contains it in abundance; appearing by their augmentation to be the first cause of the formation of calculi, and furnishing them with the gluten which unites their molecularæ, and following, in their proportion, the energy or weakness of the digestive forces of the distribution of the chylous matter.

K. The special urinary matter, giving to urine its characteristic properties really constituting it; giving it its odour, colour, and flavour—its alterability into ammonia, the carbonic and acetous acids, &c. It is the most abundant of the matters contained in urine; it forms alone the  $\frac{1}{2}$  of its matters. It has been improperly considered by Rouelle the younger as a saponaceous extract. To it is owing the almost total crystallisation of urine evaporated to the consistence of syrup; the solid and crystalline form it assumes in that state by the addition of concentrated nitric acid; the crystallisation of the muriates of soda and ammonia modified, and in some measure reversed—the former from the cube to the octædron, and the second from the octædron to the cube. This particular animal matter, which we call *uric*, will form the object of another Memoir, destined to serve as a supplement to

the present. It is of much importance to animal physiology and the art of healing.

It appears that, besides these ten substances, the real and constant materials of human urine, it contains sometimes, but rarely and accidentally, sulphat of soda, muriat of potash, sulphat of lime, oxalat of lime and of silex: that some of these substances, and particularly the two latter, are only the rare productions of some particular and perhaps morbid dispositions of urine. It will be of great utility to inquire into the circumstances which have an influence on the existence of these matters, which are foreign to the natural state.

XIV. In characterising urine as a liquid very distinct from all others, the matter which I have called *urée* gives it, above all, the property of becoming, by the decomposition which it occasions, a liquor very different from what it was when it issued from the bladder, and a substance totally new. Fermented urine is changed in almost all its matters. The view of these changes, which terminates this Memoir, exhibits, as the most striking result, the production and existence in putrid urine of nine new matters, which do not exist in fresh or natural urine.

1. Ammonia in excess.
2. The phosphoric acid saturated by this alkali.
3. The phosphat of magnesia converted into ammoniaco-magnesian phosphat.
4. The urat of ammonia.
5. The acetous acid united to ammonia.
6. The benzoic acid saturated with the same ammonia,
7. The muriat of soda become octadral.
8. The muriat of ammonia become cubic.
9. The carbonat of ammonia.

We may still add the precipitation of the albumen and gelatinous matter effected by the ammonia, and which accompanies that of the phosphats; so that these salts, like the matter of bone, are susceptible of giving carbon when heated.

Such is the general view of the facts contained in this first Memoir. They prove how many new and important results



a profound study of urine may présent to those who will bestow on it that continued attention which it requires, and what influence such phenomena may have on the progrès of animal physiology. The authors promise to make known, in a second Memoir, all the properties which distinguish the particular matter that characterises this liquid, which is the most abundant in it, and which they call *urée*, as has been already said.

XII. *Extract from the Report respecting Experiments made at the Polytechnic School in the Years V. and VI. on the Combustion of the Diamond.* By C. GUYTON\*.

THAT the diamond is combustible, is a truth which Newton, in some measure, conjectured; which experience has fully confirmed; and respecting which it is no longer possible to entertain the least doubt. But what is the nature of this combustibility? My first experiments, published in 1785, on the entire combustion in nitre in fusion †, seemed to announce that the diamond burnt in it after the manner of coal, since it left an effervescent alkali; and this suspicion acquired more reality, after the examination made by Lavoisier of the gas remaining in the vessels in which it had burnt, and which he found charged with carbonic acid. Mr. Tennant has since furnished us with a new proof of this important fact, by repeating the combustion of the diamond by nitre in a gold crucible, as I had proposed, in order to obtain a residuum absolutely free from all foreign matter ‡.

There were, however, still sufficient reasons to induce us to disbelieve that the diamond and carbon, or that the diamond and the carbon extracted from the carbonic acid by the noble experiment of Mr. Tennant, were the same substance. Independently of their external characters, so completely different, several observations, which I have already communicated to the Institute, prove that their chemical characters

\* From the *Annales de Chimie*, No. 91.

† *Opuscles de Bergman*, French edition, Vol. XII. p. 124.

‡ *Philosophical Transactions* 1797.

no less excluded this identity. Indeed if the diamond was pure carbon, why had it not the same affinities? Why does it not, like it, serve to make the oxygenated muriat of potash detonate; to deoxygenate sulphur, arsenic, and phosphorus; to deoxygenate metals, which are sufficiently fixed to undergo the degree of fire which determines its combination with oxygen? Why does it not form also carbures? Why does it not, like it, conduct the electric fluid? We know that the aggregation conceals sometimes the affinities by counterbalancing their power, but not in operations where the bodies are sufficiently fixed, and the temperature sufficiently high, to render effectual the weakest attractions. Something remained, then, to be discovered to harmonise and make us comprehend facts in appearance so contrary.\* I imagined that it was by attentive observation of what took place during the act of the combustion of the diamond, that we should attempt to penetrate this secret of nature. The experiments I am about to describe will, I hope, prove that my attempt has not been vain; that the explanation of phenomena, which have appeared to us the most incoherent, may hereafter be deduced from some circumstances which were not observed nor even suspected, and which have enabled us to make an important step in the knowledge of the nature of the diamond, since we can indicate substances which approach much nearer to it than carbon.

These experiments, begun in Thermidor year 5, were not terminated till the 11th of Fructidor year 6. Various accidents, which may be readily conceived, and the fewness of the days when the rays of the sun are not interrupted by clouds or weakened by vapours, were the principal causes of this delay. I shall suppress the details of those essays which did not lead to conclusive results; but I shall not neglect those which furnished us with an opportunity of observing several times the same facts, and sometimes in a more distinct manner; though, by the effect of some unforeseen circumstance, it was not possible to keep an account of them in

\* C. Berthollet, in his Lectures at the Normal School, still leaves in doubt whether the diamond is crystallised carbon, or carbon combined with some other substance.

regard to the correspondence of the ingredients and the products. I must not omit to remark, that I had, as co-operators in these experiments, C. Clouet and Hachette. The journal of them was correctly kept by C. Deformes, formerly a pupil of the Polytechnic School, and at present assistant-preparer for one of the courses of chemistry. The most important phenomena were seen, at different sittings, by several men of letters.

The Council of the Administration of the Polytechnic School, approving the object of the experiments, authorised me to dispose of some of the diamonds in its cabinet\*.

The first experiment was made on the 9th of Fructidor in the year, 5. We placed upon the table of the mercurial pneumatic tub, a bell of flint glass 18.3 centimetres in diameter, and of the content of 5580 cubic centimetres. Near the tub was placed, on one side, a pneumatic machine, to exhaust the common air from the bell by means of a bent tube, which rose as far as the knob. On the other side was a water pneumatic tub, bearing a large receiver, having at its tubulure a cock which communicated with the inside of the bell by a tube of bent glass, and rising, in the like manner, to the hollow knob of the bell. On one of the edges of the mercurial tub was fixed a slide, bearing a kind of mandril, destined to receive a cylinder of hard wood moveable in every direction, terminated by a handle of iron, and serving to support a cup made of the crucible earth of Valogne; so that this cup could be conveyed to every point of the interior part of the bell to present the diamond to the focus. This apparatus is represented by fig. 1. (Plate II.) Every thing being disposed in this manner, we put into the cup

\* The diamonds, which form part of its collection of minerals, were found in an English ship from Senegal, captured in the year 2. They were deposited at the *Hôtel des monnoies*, where they remained till the year 5, when, on the suggestion of our colleague Mongez, one of the administrators, the minister of the finances, after causing it to be ascertained that the greater part of them were unfit for being cut, and for that reason more valuable for instruction, ordered one third of them to be given to the Museum of Natural History, another to the Cabinet of the School of Mines, and another to that of the Polytechnic School. The lot assigned to the last consisted of 26, weighing altogether 3.662 grammes.

an incomplete octaedral diamond, having the edges a little rounded, of a dirty water inclining to yellowish-grey, and weighing exactly 142 milligrammes. On the cup, the edge of which was ground flat, a cover was applied, attached to a thread tied round the lower part of the support.

The pump was made to act till the mercury in the bell rose within less than a millimetre of the orifice of the tubes of communication. The cock of the receiver of the hydro-pneumatic tub, which had been previously filled with oxygen gas obtained from the oxygenated muriat of pot-ash, was then opened; and the first portion of this gas which had passed into the bell was extracted by the pump, in order to exhaust as much as possible the remainder of common air. After this it was filled with the same gas to within 69 millimetres of its internal edge, and 51 of the external. It may readily be conceived that still a little air remained in the cup in which the diamond was placed, and which, during these experiments, had been shut by its cover; but its content not being altogether three cubic centimetres, this inconvenience was thought the smallest of those which were to be apprehended.

The diamond having been uncovered, we began, at ten minutes after one, to throw upon it the focus of the large lens belonging to the cabinet of the Polytechnic School, the diameter of which is 40.59 centimetres, and the focus 135.3. We were well aware of the necessity of heating the bell by degrees, to prevent its cracking. For this purpose we interposed, at first, green and blue coloured glass; but whether they acquired more heat, or resisted dilatation more, these glasses all speedily broke; and we were not able to accomplish our object but by covering with paper, for some moments, the part of the bell which received the luminous disk. When the paper was withdrawn, the mercury fell rapidly, 19 millimetres in the inside. The diamond, exposed to the focus for twenty minutes, did not inflame. It appeared at first mealy, but sensibly blackened at the surface when observed through the coloured glass while exposed to the focus\*.

The

\* This carbonaceous colour had been before observed by Lavoisier in his

The focus having been intercepted by an opaque body to examine more closely the state of the diamond, no alteration was remarked in it, except that it had assumed a yellowish shade, perfectly like that of transparent amber. The atmosphere beginning to become charged with vapours, the experiment was discontinued, in order that we might resume it at a more favourable moment. This moment occurred the next day the 10th, and was announced by a rising of the thermometers; one of which, exposed to the sun in the open air, rose to 40 degrees; and the other, exposed to the sun also under a bell-glass to compare the interior temperature, rose to 44 degrees.

The focus was thrown upon the diamond at 28 minutes after 11. At 42 minutes after 11 the cone of light was intercepted, and we saw the diamond red, transparent, and surrounded with a faint radiation. When cooled, its edges appeared blunted: we observed in it a black point; but it had become white, and had lost the yellow colour acquired the day before.

The experiment could not be resumed till the 15th. We began, by noting the height of the interior column of the mercury, to calculate, according to the temperature and pressure, the volume of the æriform fluid remaining under the bell; and we judged that it had decreased about 173 cubic centimetres.

The rays of the sun were bright and strong; but the air so much agitated, that while the thermometer under the bell was at 44.5, that exposed to the sun in the open air did not rise higher than 32°. There was a moment, however, when the luminous cone produced a slight scintillation on the surface of the diamond. An opaque body, immediately interposed, made it appear red; but more obscure than on the 10th. It was also found white after cooling.

Being astonished that the diamond, when inflamed, as on the 10th, did not maintain of itself the temperature necessary for its combustion, especially in oxygen gas, as happens to

his experiments made on the diamond with the large lens of Trudaine.  
See *Dict. de Chémie de l'Encyclop. Méthod.* Vol. I. p. 741.

metallic combustibles\*, we imagined it might result from its being too much in a mass, or perhaps also too much insulated from every other combustible which might contribute to this temperature: and that we might make an attempt to remove this obstacle, we introduced into the same porcelain cup, and without deranging the apparatus, a small cut diamond of the weight of eight milligrammes; but there was no appearance that the combustion was in the least augmented; and this small brilliant, instead of being more rapidly attacked by the heat, after having been two days exposed to the action of the solar fire, capable of igniting obscurely the large diamond placed close to it, gave no signs of inflammation, and was taken from the apparatus without having experienced the slightest alteration, either in the polish of its surface, or the vivacity of its edges.

On the 23d of the same month we took the diamonds from the saucer, to examine, with care, that which had given manifest signs of a commencement of combustion. It weighed no more than 88 milligrammes; it had therefore lost 54, about 0.38 of its weight. It still retained its original octahedral form; but the angles were blunted, and the edges rounded. The surface was tarnished, and full of small inequalities; which, observed with a magnifying glass, presented cavities, salient points, and sometimes parallel sections of the laminæ. In several of the cavities we could plainly perceive a sort of specks inclining to grey; but what appeared worthy of most attention was, a pretty large hollow almost at the extremity of one of the quadrangular pyramids, which seemed to indicate the place where the solar focus had exercised, at the end of the operation, its greatest intensity; and where we distinguished a blackish stripe, not terminated like a stroke formed by a foreign body, but on the contrary softening itself off, and penetrating into the interior part of the mass by degrading its colour.

\* We had the more reason to be surprised at this phenomenon, as M. Landriani had announced that the diamond, inflamed by brass wire, burnt like it in oxygen gas, and with the utmost brightness: he, indeed, excepted the Brazilian diamonds, which he was not able to inflame by these means. *Annales de Chimie*, Vol. XI. p. 156.

I thought it might be of some importance to preserve the subject of these observations, and that it would not disgrace the collection of the cabinet of the School, with an inscription allusive to the experiment to which it had been subjected. Another diamond was therefore destined to be put into the apparatus, in order that it might be there subjected to entire combustion. This diamond was also a pretty regular octaedron, of a much more beautiful water than the preceding, and weighing 200·1 milligrammes, 3·77 grains.

As the season was already so far advanced that it left us no hope of a solar focus as strong as that from which we had obtained so little effect with the lenses we had employed, I was desirous of terminating the experiment with the great lens of Tschirhausen; and the class granted me permission to take it from their cabinet.

This lens, as is well known, is 86·6 centimetres (32 inches) in diameter, and 211·076 (73 inches) focus. We augmented its power still more by catching the luminous cone with the small lens of the cabinet of the Institute, the disk of which is 37·89 centimetres, and the focus 56·83, which in this position was shortened to 5·41 centimetres.

A first sitting gave scarcely any signs of a commencement of combustion. Next morning, the luminous disk having fallen on one of the parts of the bell which was thickest, it occasioned it to crack. It was therefore no longer possible to compare the volume of the gas before and after the operation, nor to distinguish and ascertain the quantities of the products. We confined ourselves to making lime-water pass through the interior of the bell before the fissure had suffered a sensible quantity of common air to enter, and we observed that it was much troubled.

The diamond which had been last exposed was noways changed at its surface: it had, however, lost two decimilligrammes of its weight; which was verified by the same balance with which it had been weighed, and which is capable of marking, in a very sensible manner, these fractions of the milligramme. Thus we were obliged to adjourn the experiment till the next summer, in order to find a more favourable sun, and to have time to provide a new apparatus.

[To be continued.]

XIII. *Account of the Processes used by Mr. SHELDRAKE to separate the Mucilage from Linseed Oil, and to dissolve Copal in Spirit of Turpentine, and in Alcohol\*.*

*To separate the Mucilage from Linseed Oil.*

I HAD read in some book, the title of which I do not recollect, that linseed oil might be purified by shaking it with water, which would imbibe the impurities, and leave the oil more limpid. I tried this experiment by shaking linseed oil with warm water; and was surprised to find they did not separate, but remained united in the form of emulsion. I then boiled them together, and found their tendency to separate diminish. As it is the known property of gums or mucilage to keep oil and water united in this state, I was induced to suspect the presence of mucilage in linseed oil.

I had read in Doffie a method of preparing what he calls *fat oil*. It is effected by placing linseed oil in a shallow vessel exposed to the heat of the sun, and stirring it frequently: in a certain time it loses its property of drying, thickens, and acquires a degree of tenacity that makes it proper for a size or cement for gilders, &c. A similar substance is alluded to by Leonardo da Vinci.

Taking the existence of mucilage in this oil for granted, I conjectured that the alteration, produced in its texture by Doffie's process, arose from the evaporation of some of its principles, and the more intimate union of the rest in consequence of that evaporation. To verify this conjecture, I tried the following experiment:

I filled a half-pint phial full of linseed oil, corked and tied it securely over with a bladder. This I exposed to the heat of the sun in summer, during the whole day: after it had remained a few days in this situation, the upper part of the

\* From the *Transactions of the Society for the Encouragement of Arts, &c.* Vol. XVII. The present may be considered as a continuation of Mr. Sheldrake's paper on Painting in Oil, in a manner similar to that practised in the ancient Venetian School, published in Vol. XVI.: for a copy of which see *Phil. Mag.* Vol. I.



phial was covered with drops similar to those produced by holding a wet bottle to dry before the fire. I then shook it well, which made the contents look muddy, and set it to rest again. After a time it became clear, and a portion of transparent liquor, like water, lay at the bottom. I then repeated the shaking and setting it to rest, till no additional quantity of this fluid was separated.

By trying this experiment repeatedly upon oils procured from different places, I found that some oils afforded much more mucilage than others. From some I separated a third part of mucilage; from others, a pint would not afford more than a table-spoonful, and sometimes less. Whether this difference in the result was radically in the oils, or from a difference in the processes conducted by means so variable as the heat of the sun, I am not able to ascertain.

Upon trying the same experiment with nut and poppy oil, I found the same result, but in a different degree. The average quantity afforded by nut oil was, I believe, not more than a third part of the average of linseed oil; and the average of the poppy oil was not so much as a sixth. In some cases, particularly of the poppy oil, I did not obtain any.

The colour of the oil always diminished as the mucilage was abstracted; but the mucilage was always as colourless as water. It is a question I will not pretend to decide, whether the colour of these oils depends upon the presence of the mucilage, or upon any other principle which is destroyed by the action of light. I have in some instances had the oil as colourless as water.

This decomposition of the oils, if it may be so called, is curious, as proving the mucilage in them; but, as it is very troublesome, may it not be advisable to prefer those which have naturally the least mucilage in their composition?

After pouring the oils from the mucilage, I put several quantities of the latter together, and found them mix without any difficulty. I mixed this mucilage with water, and found it unite with it in any proportion without becoming turbid. I laid it upon plates of metal, exposing them to dry in the sun and before the fire, and, when dry, washed them with a sponge and water: but it shewed no tendency to dis-

solve; though, while in a liquid state, it seemed to possess all the properties of a gum.

As the processes by which I dissolved the amber and copal to make the oil varnishes, are to be found in many books, and as it will be better for artists to purchase than attempt to make those varnishes, it can scarcely be thought necessary to detail those processes here: but, as I believe the methods by which I dissolved the copal in spirit of turpentine and spirit of wine are not known, I shall now subjoin them.

*To dissolve Copal in Spirit of Turpentine.*

N. B. Whatever quantity is to be dissolved, should be put into a glass vessel capable of containing at least four times as much, and it should be high in proportion to its breadth.

Reduce two ounces of copal to small pieces, and put them into a proper vessel. Mix a pint of spirit of turpentine with 1-8th of spirit of sal-ammoniac; shake them well together; put them to the copal; cork the glass, and tie it over with a string or wire, making a small hole through the cork. Set the glass in a sand-heat so regulated as to make the contents boil as quickly as possible, but so gently that the bubbles may be counted as they rise from the bottom. The same heat must be kept up exactly till the solution is complete.

It requires the most accurate attention to succeed in this operation. After the spirits are mixed, they should be put to the copal, and the necessary degree of heat be given as soon as possible. It should likewise be kept up with the utmost regularity. If the heat abates, or if the spirits boil quicker than is directed, the solution will immediately stop, and it will afterwards be in vain to proceed with the same materials; but if properly managed, the spirit of sal-ammoniac will be seen gradually to descend from the mixture and attack the copal, which swells and dissolves, except a very small quantity which remains undissolved.

It is of much consequence that the vessel should not be opened till some time after it has been perfectly cold. It has twice happened to me, on uncorking the vessel when it was not warm enough to affect the hand, that the whole of the contents were blown with violence against the cieling. It is  
likewise

likewise important that the spirit of turpentine should be of the best quality. I have never succeeded with that which is sold at the colour-shops; but whenever I procured my spirits at Apothecaries' Hall, I have dissolved the copal, by the process I have described, without difficulty.

This varnish is of a rich deep colour when viewed in the bottle, but seems to give no colour to the pictures it is laid on: if left in the damp, it remains sacky, as it is called, a long time; but if kept in a warm room, or placed in the sun, it dries as well as any other turpentine varnish; and when dry, it appears to be as durable as any other solution of copal.

*To dissolve Copal in Alcohol.*

Dissolve half an ounce of camphire in a pint of alcohol; put it in a circulating glass, and add four ounces of copal in small pieces; set it in a sand-heat, so regulated that the bubbles may be counted as they rise from the bottom; and continue the same heat till the solution is completed.

Camphire acts more powerfully upon copal than any substance that I have tried. If copal is finely powdered, and a small quantity of dry camphire rubbed with it in the mortar, the whole becomes in a few minutes a tough coherent mass. The process above described will dissolve more copal than the menstruum will retain when cold. The most economical method will therefore be to set the vessel which contains the solution by for a few days; and when it is perfectly settled pour off the clear varnish, and leave the residuum for a future operation.

This is the brightest solution of copal that I have seen: it is an excellent varnish for pictures, and may perhaps be found to be an improvement in fine Japan works; as the stoves used in drying those articles may drive off the camphire entirely, and leave the copal pure and colourless on the work.

N. B. Copal will dissolve in spirit of turpentine, by the addition of camphire; with the same facility, but not in the same quantity, as in alcohol.

At the time I determined to lay the preceding papers before the Society, I conceived that the quick and cerain

manner in which the vehicle dried, was one of its advantages. But as that circumstance has been objected to, and in some cases really is a disadvantage, I have since endeavoured to remove that objection by the following process:

Put a pint of nut or poppy oil into a large earthen vessel; make it boil gently upon a slow fire; put in by degrees two ounces of ceruse, and stir it continually till the whole is dissolved.

Have ready a pint of the copal oil varnish heated in a separate vessel; pour this by degrees into the hot oil, and stir them together till all the spirit of turpentine is dissipated; let it then be set by till cold, when it will be fit for use.

It is obvious, that, as this is a compound of the copal varnish with the least exceptionable of the drying oils, it will partake of the properties of each of its component parts. It gives less brightness and durability to colours than the varnish will, but more than oil: but as it certainly may be used in painting in the same manner as any other drying oil, and gives more brightness and durability to colours than they can derive from any other oil, it is not unreasonable to suppose that it will prove an advantageous vehicle.

I have mentioned specific quantities of the ingredients; but it is easy to see that the relative proportions may be varied according as it is required to dry faster or slower. It must be remarked too, that whenever the mixture is to be made, both the ingredients should be hot; because, if either of them is cold, the mixture becomes turbid, and a part, often the whole of the copal, is precipitated: but this inconvenience is avoided by mixing and boiling them together, as I have directed. It must likewise be observed, that after some time a spontaneous alteration takes place, which diminishes, and at last destroys the drying quality of this mixture: it will therefore be advisable to use it fresh, or at least not to use it after it has been made more than a month or six weeks.

#### XIV. *Process for producing the Lights in Stained Drawings.*

*By Mr. FRANCIS NICHOLSON, of Rippon, Yorkshire\*.*

THE difficulty of preserving the lights in stained drawings, with freedom and precision, is so universally felt by those who cultivate that branch of the arts, the practice of which is every day growing more extensive, that the statement of this circumstance alone is sufficient for the introduction of the following process, by which that difficulty is removed, and by which all the effect of body-colour may be obtained without any of its inconveniences or defects. It is applicable to every subject, to the richness of foliage, of rocks, or of foreground; and in ruins, their most picturesque appendages of hanging shrubs, weeds, &c. may be expressed by it with the utmost sharpness, and with all the lightness and freedom of which body-colour or oil-painting are capable.

The principle of this process consists in covering the places where the touches of light are intended to be, with a composition not liable to be displaced by washing over it with the colour, and such as may be afterwards removed by a fluid in which the colours used in water are not soluble.

This composition, or stopping mixture, is made by dissolving bees-wax in oil of turpentine, in the proportion of one ounce of wax to five ounces of the oil; and, as near the time of using it as may be convenient, grind with the pallet-knife as much flake white, or white lead, in oil of turpentine, as may be wanted at one time; dilute it with the above solu-

\* The Society for the Encouragement of Arts, &c. having last session received from Mr. Nicholson, of Rippon in Yorkshire, a Drawing intended as a specimen of the process for producing the lights in stained drawings, by removing, after the shadows are washed in, the colour where the lights are required, giving by this means the effect of body-colour with greater clearness, and without any of its disadvantages; and it appearing that Mr. Nicholson's method of tinting drawings promises to be of use in the practice of drawing in water-colours, and produces a more spirited effect, the Society agreed to Mr. Nicholson's proposal, and purchased from him, at the price of twenty guineas, the complete process for performing the work, as communicated in the above paper.

tion until it will work freely with the pencil, and appear on the paper, when held between the eye and the light, to be opaque. It is necessary to observe this, or the first touches will not be sufficiently visible, after being washed over with the colours, to ascertain the places of the second. It is also necessary to use a frame instead of the drawing-board, or to paste the paper on the frame of the drawing-board so as to remove the pannel; because the first and second touches must be put on with the drawing placed between the eye and the light, as they will be most visible in that situation. On this frame paste the paper wet, so as to dry firm: when quite dry, draw the outline, and proceed as follows:—

1st, With a fine small hair pencil, and the stopping mixture, cover those places where the clear whiteness of the paper may be wanted, except in the sky: let it dry a few minutes; then wet the paper on both sides, and while it is wet wash the sky. The shadows of the clouds, distances, and general breadths of shadow, must be put in with the grey tint; and over the places of the light, wash the tints of the brightest light; those will be generally yellow ochre or light red.

The light of the clouds may be preserved sharp by pressing on that part a piece of tissue-paper previous to the washing of the sky; this, by absorbing the superfluous moisture, will prevent the colour from spreading farther than is desired. Suffer the whole to be very dry; and,

2dly, Touch in with the stopping mixture, the sharp and prominent parts of the brightest lights; let them dry a few minutes, then wash over them with the tints of the next degree of light.

3dly, Stop with the mixture the second order of touches, and wash over them with the middle tints; strengthen also at the same time the breadths of shadow.

4thly, Stop, with broad touches of the mixture, the places of the middle tint; uniting them to the former touches, and extending them so as to graduate the middle colours into the shadow: strengthen the shadows, making them nearly as dark as they are intended to be, and let the whole be perfectly dry.

Then

Then take oil of turpentine, and with a sponge, or hog's-hair pencil, wash over the places where the mixture has been used, rubbing it with the brush until it be dissolved: clear it away with a linen rag, and wash it with more oil of turpentine so long as any white lead appears; then let it dry.

Warm the drawing; then with a soft brush and highly-rectified spirit of wine wash the places where the oil of turpentine has been used, to clear away the remainder of it: rub the drawing lightly on the face, but sponge it well on the back.

When dry, tint down the lights where it may be wanted; harmonise the colouring, and cut the shadows to effect, with still darker tints as may be necessary.

If other touches of light should afterwards be wanted in the shadowed parts, the colour may be easily removed by a pencil formed of sponge, with water sufficient to produce them with as much strength as can be desired; then stop them with the mixture; wash the shadow over the touches, bringing it to the colour taken off; and, when dry, remove the mixture with the oil of turpentine and spirit of wine.

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XV. *An Account of Mr. BROWN's Travels through Egypt and Syria, &c.*

[Continued from page 414 of the last Volume.]

**D**URING three or four days ensuing, Mr. Brown suffered so violent a relapse as to be unable to perform the common offices of life, and even to suppose that it was nearly at an end. The moment any symptoms of amendment appeared, he sent word to the Melek that he wished to be introduced to the Sultan, and then, as soon as possible, to be dismissed. No reply was made to this message; but the following day he came to the tent, with some of his attendants, and desired to see the merchandise he had brought with him. As to part of the articles, consisting of wearing apparel, &c. suited to the great, our traveller readily consented: but this was not sufficient; the Melek insisted on seeing the contents of a small chest, which chiefly contained articles useful to himself, but  
not

not designed for sale. There were also in it some English pistols, which he intended to avail himself of as presents at Sennaar, or wherever else he might be able to penetrate. Mr. Brown, therefore, positively refused to open the chest: the Melek threatened to have it broke open; and, as his attendants were proceeding to do so, Ali Hamad, the man who attended Mr. Brown, took the key from its concealment and opened the box. Every thing was pulled out and examined, and many small articles appeared no more: the pistols were reserved to be taken by the Sultan, after a violent but fruitless altercation at the valuation made by his own servants; and Mr. Brown's telescopes, books of which they knew not the use, with his wearing apparel, &c. were graciously left him. The valuation was made the following day: the whole was estimated at thirty-eight head of slaves, being at the market price worth eighty, exclusively of a present of value for the Sultan. A pair of double-barrelled pistols, silver mounted, which cost in London twenty guineas, were valued at one slave; which can in general be purchased, by those who are experienced in that traffic, for the value of fifteen piastres in Egyptian commodities. On this Mr. Brown exclaimed, that if they meant to plunder, and if bargain and sale were not conducted in the country by the consent of the parties, but by force, it would be better to take the whole gratis. No answer was made; but the day following two camels were brought him as a present.

The violent manner in which our traveller's property had been seized, and the general ill-treatment he received, had much augmented his disorder. He had been fifteen days in the tent exposed to great variations of temperature; and it being at the close of the rainy season, he could rarely obtain water to drink, though tormented with thirst. He judged, therefore, that the only means of restoration were, to return to Cobbé, and avail himself of the shelter of a clay house and privacy, the want of which he had so sensibly felt. The Melek, being in possession of the greater part of his property, having left him only as much as would supply the wants of a few months, did not seem very anxious about his stay. Mr. Brown hired therefore two Arabs, and with the camels given



to him, and the property that remained, arrived on the third day at the place from which he had come. In the intervals of his illness he visited the chief persons of the place; and as the eyes of the people became habituated to him, he found his situation growing somewhat more tolerable. Though idle during the course of the winter respecting the immediate objects of his voyage, he grew, of course, more familiar with the manners and particular dialect of the country; for the Arabic spoken in it differs materially from the vernacular idiom of Egypt.

The following summer (1794) Mr. Brown, having in some degree recovered his strength, determined to go and reside for a time near the Sultan, both to supplicate for redress of what he had already suffered, and to embrace any opportunity that might offer of pressing his request for permission to advance. On his arrival at El Fasher, his good friend Melek Misellim being employed by his master in the south, he went under the protection of the Melek Ibrahim, one of the oldest persons in authority there, and took up his lodging in the house of a man named Musa. During Mr. Brown's stay at El Fasher, of three entire months, he was solicitous to attend regularly the levees of the Sultan, which were from six in the morning till ten; but could very rarely obtain admittance, and when he did he had no opportunity of speaking. After waiting in fruitless expectation at El Fasher, when the time of his departure was drawing near, an accident happened, which, though not of the most pleasing kind, contributed to make him noticed, and obtained for him at length an interview with the Sultan. One day, as he was reading in his hut, a female slave belonging to the house, a girl about fifteen, came to the door of it, when, from a whim of the moment, he seized the cloth that was round her waist, which dropped, and left her naked. Chance so determined that the owner of the slave passed at that time, and saw her. The man immediately threw his turban on the earth, and exclaimed—Ye believers in the Prophet hear me! Ye faithful avenge me! (with other similar expressions:) a Caffre has violated the property of a descendant

of Mohammed ! When a number of people was collected around him, he related the supposed injury he had received in the strongest terms, and exhorted them to take their arms and sacrifice the Caffre. He had charged a carbine, and affected to come forwards to execute his threats, when some one of the company, who had advanced farthest and saw Mr. Brown, called out to the rest that he was armed, and prepared to resist. It was then agreed among the assembly that some method of punishment might be found which promised more security and profit to the complainant, and would be more formidable to the guilty. The man who acted as his broker was to take the slave as if she had really been violated, and agreed to pay whatever her master should charge as the price. The latter had the modesty to ask ten head of slaves ; and if Mr. Brown carried the matter before the Cadi, which he supposed he would hardly venture to do, he had suborned witnesses to prove that he had received of him property to that amount.

On Mr. Brown's removal from Cobbé to El Fasher, he had caused his small remaining property, among which were a few articles of value, but many of much use to him, to be lodged in the house of Houssein, the owner of the slave, and his companion. On his return thither, which happened a few days after the accident, he claimed it ; but they resisted, as they alleged, at the suit of his broker, and would not deliver it till the value of ten slaves should be paid to him. Mr. Brown from the first considered their conduct as so violent that if it reached the ears of government the claim would unquestionably be abandoned ; and, indeed, his adversaries had rested their expectations only on the timidity which they had been accustomed to observe in Christians of the country, whose accusation and condemnation are in fact the same. He had not neglected to give the transaction all the notoriety he could without having recourse to public authority, and those to whom he had applied were decidedly in his favour. He therefore now went to his adversaries, Houssein and his companion, and in their presence offered to Ali Hamad a promissory note for the value of ten slaves at the market-

market-price on his arrival at Kahira. It was however refused; and his chest, containing some German dollars and other articles, was still detained. The rest was given up.

Mr. Brown had been told that the Sultan was apprised of the transaction previously to his departure from El Fasher, and that he intended to grant him redress; but after waiting about fifteen days without hearing any thing farther of his intentions, being weary of suffering, he determined to return. He had been arrived but a short time when a *fulganaway*, or messenger, came express from Court, with orders for him to repair to El Fasher immediately. The object of the message was kept a profound secret, nor could he discover whether it portended good or evil. He left Cobbé the same evening, and arrived at the end of his journey the day following about noon. He repaired as before to the Melek Ibrahim, who on the following day introduced him at the public audience. The Sultan, as he retired to the palace after it was over, ordered all the parties to appear. Being come within the inner court, he stopped the white mule on which he was mounted, and began a short harangue, addressing himself to Hossien and Ali Hamad Mr. Brown's servant, in which he censured, in a rapid and energetic style, their conduct towards him:—"One," said he, turning to Ali, "calls himself Wakil of the Frank; if he were a Sherif and a Muslim, as he pretends, he would know that the law of the Prophet permits not a Muslim to be Wakil to a Caffre: another calls himself his friend: both are agreed in robbing him of his property, and usurping the authority of the laws. Henceforth I am his Wakil, and will protect him." He then ordered the parties to repair to the house of Musa Wul-lad Jelfûn, Melek of the Jelabs, under whose appropriate jurisdiction are all foreign merchants. Mr. Brown here gives the following account of the manner in which he had been before received by the Sultan:

"On my first audience," says Mr. Brown, "I was too ill to make much observation. He was seated at a distance from me; the visit was short; and I had no opportunity of opening a conversation. He was placed on his seat (*cârî*) at the door of his tent. Some person had mentioned to him

my watch and a copy of Erpenius's grammar I had with me. He asked to see both; but after casting his eyes on each, he returned them. The present I had brought was shewn him; for which he thanked me, and rose to retire.

“ During the following summer the first time I got admission to him he was holding a divan in the outer court. He was then mounted on a white mule with a scarlet *benish*, and had on his head a white turban; which however, together with part of his face, was covered with a thick muslin. On his feet were yellow boots; and the saddle on which he was seated was of crimson velvet, without any ornament of gold or silver. His sword, which was broad and straight, and adorned with a hilt of massy gold, was held horizontally in his right hand. A small canopy of muslin was supported over his head. Amid the noise and hurry of above a thousand persons, who were there assembled, I was unable to make myself heard, which the nature of my situation enabled me to attempt, though not exactly conformable to the etiquette of the court, that, almost to the exclusion of strangers, had appropriated the divan to the troops, the Arabs, and others connected with the government.

“ On another occasion I contrived to gain admittance to the interior court by a bribe. The Sultan was hearing a cause of a private nature, the proceedings on which were only in the Furian language. He was seated on a kind of chair, which was covered with a Turkey carpet, and wore a red silk turban; his face was then uncovered; the imperial sword was placed across his knees; and his hands were engaged with a chaplet of red coral. Being near him, I fixed my eyes on him in order to have a perfect idea of his countenance; which, being short-sighted, I had hitherto scarcely found an opportunity of acquiring. He seemed evidently discomposed at my having observed him thus, and the moment the cause was at an end he retired very abruptly. Some persons to whom I afterwards remarked the circumstance, seemed to think that his attendants had taught him to fear the magic of the Franks, to the operation of which the habit of taking likenesses is imagined by some of the Orientals to conduce. He is a man rather under the middle size, of a complexion

complexion adust or dry, with eyes full of fire, and features abounding in expression: his beard is short, but full; and his countenance, though perfectly black, materially differing from the negro: though fifty-five years of age, he possesses much alertness and activity.

“At another of my visits I found him in the interior court, standing with a long staff tipped with silver in his right hand, on which he leaned, and the sword in his left. He then had chosen to adorn his head with the folds of a red silk turban, composed of the same material as the western Arabs use for cincture. The Melek Ibrahim presented him, in my name, with a small piece of silk and cotton of the manufacture of Damascus. He returned in answer, *Barak ulla fi!* May the blessing of God be on him! a phrase in general use on receiving any favour; and instantly retired, without giving me time to urge the request, of which I intended the offering should be the precursor. It is expected of all persons, that, on coming to El Fasher, they should bring with them a present of greater or less value according to the nature of the business in hand. It is no less usual, before leaving the royal residence, to ask permission of the Sultan for that purpose. With this latter form, which was to me unpleasant, I sometimes complied, but more frequently omitted it. But on this occasion, having been long resident there, I thought fit to make a last effort to promote my design. The day preceding that which I had fixed for my return, happened to be a great public audience. I found the monarch seated on his throne (*cârî*) under a lofty canopy, composed not of one material, but of various stuffs of Syrian and even Indian fabric, hung loosely on a light frame of wood, no two pieces of the same pattern. The place he sat in was spread with small Turkey carpets; the Meleks were seated at some distance on the right and left; and behind them a line of guards, with caps ornamented in front with a small piece of copper and a black ostrich feather. Each bore a spear in his hand, and a target of the hide of the hippopotamus on the opposite arm: their dress consisted only of a cotton shirt of the manufacture of the country. Behind the throne were fourteen or fifteen eunuchs, clothed indeed splendidly in habiliments of cloth

or silk, but clumsily adjusted, without any regard to size or colour. The space in front was filled with suitors and spectators to the number of more than fifteen hundred. A kind of hired encomiast stood on the monarch's left hand, crying out a *plein gorge* during the whole ceremony:—"See the buffalo, the offspring of the buffalo; a bull of bulls; the elephant of superior strength; the powerful Sultan Abd-el-rachmân-el-rashîd! May God prolong thy life, O master! May God assist thee, and render thee victorious!" From this audience, and those which preceded it, I was obliged to retire as I had come, without effecting any thing."

Mr. Brown's reception with Mufa Wullad Jelfûn was very different from that which he had experienced in the house of Misellim or Ibrahim. His behaviour towards him was complacent, and he affected to seek opportunities of hearing his sentiments on such subjects as occurred. In obedience to the Sultan's command, Mr. Brown now gave in an exact statement of the property he had lost, and substantiated the proof by the strongest circumstantial evidence. With regard to the slave, the most complete redress was afforded him: the charge brought against him was judged absolutely futile, and she was restored to her master; while he, on the other hand, was compelled to give up the chest, &c. which had been violently with-held. The plunder which had fallen into the hands of his servant, and his accomplice, was not so easily restored. The Melek, tired of a gratuitous justice, began to think that a lucrative composition was more eligible. The offenders, who had been obstinate in the first instance, seeing how the cause relative to the *jarea* (female slave) had been decided, thought proper to offer to the Melek marks of their gratitude for the lenity they expected from him; and the Sultan was unwilling to imagine that the sufferings of a Caffre could fall heavy on himself at the day of final retribution. At length the Melek, who in reality was supreme arbiter of the contest, contented himself with giving Mr. Brown the intrinsic value, about four head of slaves, instead of twenty-four or twenty-five, which at first he had unequivocally declared due to him, and promised he should receive. Thus the matter was terminated,

[To be concluded in the next Number.]

XVI. Dec.

XVI. *Description of the Paliorum Lacus, or Lake Palius, in the Valley of Noto in Sicily.* By M. DOLOMIEU\*.

THIS small but very remarkable lake, situated at the distance of two miles west from the town of Palagonia, or of a mile from Mineo, is surrounded by volcanic mountains, and lies in a small plain somewhat hollow towards the middle. This plain is half surrounded by steep rocks, which give it the appearance of a monstrous crater, that has sunk down by some convulsion. The lake is placed in its centre, and, as it were, in the middle of a funnel. Its depth often varies, and consequently its circumference. In the winter time it may be about sixty or seventy fathoms in diameter, and about ten in depth; but in summer, when great drought prevails, it is often entirely dry. At the period when I saw it, which was in the month of May, it formed an oval about thirty fathoms in length and twenty in breadth: its depth was about five or six. It had a strong smell of Jew's pitch, or asphalt, even at a considerable distance. The water in its colour inclined a little to green, and had an exceedingly nauseous and disagreeable taste. I was told that the water was often tepid; but it had, at the time when I examined it, the temperature of the atmosphere. In several parts of it I observed a violent bubbling, and particularly in four places near the middle. This bubbling was stronger at certain intervals, and the water was thrown up sometimes to the height of two or three feet, rising in this manner every five or six minutes. There are periods when this boiling is stronger or weaker; but on these occasions no other noise is heard than that produced by the motion of the water.

When the lake is dry, one may without danger examine its centre, where there are several deep holes. From these holes currents of air, somewhat warm, continually arise, and throw back sand and other bodies if put into them. It is this æriform vapour which, when the basin is full, forces up the water as before described, and makes it to be covered

\* From *Magazin für das neueste aus der Physik*, Vol. III.

with

with foam. It would, no doubt, be important to examine accurately the nature of this vapour; but though I was provided with the proper apparatus, I could not obtain my object, because I durst not venture to wade into the water in order to reach the places where the bubbling appeared, and which were at a considerable distance from the bank.

The mud at the bottom and on the bank, which has a black colour, is tenacious, and smells like pitch. A little petroleum has also been sometimes found at the surface of the water. The whole soil of the small plain consists of black, tough, resinous, inflammable earth. A few years ago some straw huts in this neighbourhood having been set on fire, the fire was communicated to the ground, which burnt with a whitish dull flame, like that of the inflammable springs in Dauphiny, during several months, and was extinguished with the greatest difficulty, as the fire, when destroyed in one place, broke out in another. Since that period great care is taken not to kindle fire in the neighbourhood. From this phenomenon I am induced to think, that the air which rises through the water of the lake, and which probably finds a passage through the ground, may be of an inflammable nature also; as the air of marshes, which burns without any noise. The fertility of this small plain is so great, that it produces a most abundant crop every year, without requiring much agricultural care. In walking over the ground a hollow noise is heard; which seems a proof of there being beneath the surface subterranean cavities, like those of Solfatara near Puzzuolo. From these circumstances there is reason to think that this place has been formed from the remains of a fallen crater, a part of which is still seen in the surrounding mountains; and between this lake and that of Agnano near Naples, there is no other difference than the greater quantity of water in the one, and the more violent efflux of vapour in the other. Some assert that the vapour of this lake is mortal; and that no bird, or other animal, can be exposed to it without being killed. The vapour also which arises from the ground is considered as of a suffocating nature, so that people who lie down on the earth, or only bend their bodies towards it, are exposed to the greatest danger; though one



may walk over it without the least fear of any pernicious consequences. All these phenomena have a great similarity to those of the Grotto del Cane, near the lake Agnano.

On the banks of the lake Palius there are found a great many small clusters of ashes and slag, exactly like those adhering to the sides of the craters of *Ætna* and *Vesuvius*. The mountains and lava around this lake exhibit the most evident marks of their great antiquity, as in many places they are covered with calcareous stones, from which it may with certainty be inferred that they must have originated at a time when the present continent was not inhabited. In the time of *Diodorus Siculus*, however, the crater, which forms the present basin of the lake, shewed traces of its internal inflammation: as we are informed by this historian, that flames burst forth from this spot; that the water possessed a considerable degree of warmth; and that a horrid thundering noise was heard. Being extremely desirous to know whether a connection actually existed between this volcano and that of *Ætna*, as they are at no great distance from each other, I enquired whether any affinity had ever been observed between the eruptions of *Ætna* and the before-described phenomena of the lake; and whether, during these eruptions, a stronger ebullition had taken place: but I was assured that no person had ever observed any thing of the kind.

The phenomena of this lake have at all times been attended with so many singularities as to give rise to fables without number: at present it is said to be inhabited by a fairy. In ancient times these phenomena were ascribed to the supernatural power and influence of some deity. On this account a very celebrated temple, the remains of which I could not find, notwithstanding all the trouble I took to discover them, was erected here to *Jupiter* and the nymph *Thalia*. People swore also by this lake, with the same respect and solemnity as by the river *Styx*. What astonished the ancients, and what even at present excites the wonder of all those who visit this lake, is the incessant ebullition of the water, though the least increase is not observed in it on the bank.

In the neighbouring mountains I found under the lava a  
refinous

resinous strong-smelling substance in horizontal strata, which could be easily divided into laminæ of from one to two inches in thickness; but, in other respects, the lava of this volcano exhibits nothing but what has been seen in the other extinguished volcanoes in the neighbourhood. The Val di Noto is the only part of Sicily where traces of ancient volcanoes are found. Travellers, who imagined that they found some of the like kind in other provinces, were either mistaken, or suffered themselves to be deceived by false marks.

XVII. *Observations on the Nature of the Fog of 1783.* By M. DE LAMANON, *Correspondent of the Academy of Sciences at Paris* \*.

WHEN this fog, which may be called an electric fog, began to appear, I was at Sallon de Crau, in Provence. In order to free my neighbours and countrymen from uneasiness as much as possible, I wrote a letter to M. Artaud, editor of the *Courier d'Avignon*, in which, after speaking of the nature of the fog, I said it would be destroyed by the storms that would not fail to ensue. The event fully justified this kind of prediction. Having learned from the public papers that this phenomenon was not local, but almost general throughout Europe, I made new observations, and traversed the highest Alps of Provence, Dauphiny, and Piedmont; and, during the course of my travels, collected information respecting the fog, and the effects of the thunder. But before I offer that explanation of the phenomenon, which appears to me most probable, let me endeavour to give an accurate description of it.

### *I. Nature and Effects of the Electric Fog.*

1. In almost all countries the fog was preceded by a storm.
2. It began the same day at places very remote from each other; as Paris, Sallon, Turin, Padua, &c. where it appeared, for the first time, on the 18th of June. M. Senebier

\* From the *Journal de Physique*.

wrote to the Count de Saluces, perpetual president of the Academy of Turin, that the fog was observed at Geneva on the 17th. I read in the *Affiches* of Dauphiny, that it began at Grenoble on the 21st.

3. A north wind prevailed in several places when the fog began; and in other places, where it began the same day, a south wind prevailed. The fog, after having ceased, re-appeared in some places by the north wind; in others, by an east, west, or south wind.

4. The atmosphere was not every where equally dry. At Sallon I observed that it did not make salts enter into deliquescence; did not raise the hygrometer; did not prevent evaporation from being abundant; and did not even tarnish glass, which I exposed to it. The salt-pits at Hyeres, in Provence, crystallised a fortnight sooner than usual by the effect of the fog. Messrs. Toaldo and Senebier observed, the one at Padua and the other at Geneva, that the hygrometer did not reach the point which denotes humidity. In the Champsaure of Dauphiny, and at Turin, the fog was sometimes humid.

5. The sun, which was never seen but through the fog, appeared very pale in the day-time; of a blood-red colour at rising, and still more so at setting.

6. At Sallon the fog sometimes diffused a very disagreeable smell, difficult to be determined, and which some believed to be sulphureous. This bad smell was perceived in other places.

7. It was hurtful to the eyes. At Sallon, persons whose lungs were weak, found disagreeable effects from it. The inhabitants of the Champsaure informed me that several people in that neighbourhood had violent pains in the head; and that, in general, they partly lost their appetite. The inhabitants of this valley are the greatest eaters in France: strangers, who reside among them, and drink their waters, eat almost as much as they, and are not so nice as usual in regard to the quality of their food. This I experienced in 1783.

8. In Lower Provence, Languedoc, and other places, the fog ripened the corn, and was favourable to the harvest. The

peasants beheld, with the greatest satisfaction, the effect of the fog on their crops; and yet were afraid of it. In Upper Dauphiny, and at Turin, it blighted several fields of wheat, and rendered copper buttons green. In other places it dried the plants.

9. At Padua, Turin, Paris, Sallon, Grenoble, the barometer remained almost always stationary at the point which denotes its mean state.

10. There were some days remarkably warm; but, in general, the months of June and July were almost everywhere less warm than usual. That year there was no summer on the high mountains of Provence and Dauphiny, where the shepherds of the plains of Crau and Camargue tend annually numerous flocks.

11. There were every where storms of rain; and after these storms the fog sometimes increased, but most frequently was diminished.

12. During the whole time of the fog, an electric machine I had at Sallon emitted few or no sparks. A philosophic friend at Sorgues, near Avignon, wrote to me, that his electrometer always indicated a great deal of electricity in the atmosphere.

13. On the 4th of July, at five in the morning, M. Nicolas, physician at Grenoble, and M. Plana, apothecary, took four measures of fog and mixed them with two measures of nitrous air: the absorption was 1-4th, and nothing remained but a gas, in which a candle became extinguished several times. Atmospheric air generally contains nearly 1-3d of pure air (oxygen gas), and 2-3ds of mephitic air (azotic gas). Fontana's eudiometer gave the same result on the 7th of July: of three hundred parts of atmospheric air, thirty-two were absorbed. The air of the fog, mixed with inflammable (hydrogen) gas, did not prevent it from exploding when a lighted taper was presented to the neck of the bottle in which it was contained.

14. Almost all those who have spoken of this fog, say that it was low. When I was on the top of Mount Ventoux, however, nearly 1040 toises above the level of the sea, I saw it far above me. M. Senebier says, in his letter to Count

de Saluces, that it was seen in the Alps at an elevation greater than that of Mount Salève, which rises 601 toises above the sea. On the 22d of September (1783) I ascended the highest Alps of Dauphiny, to the height of 1660 toises above the sea. (No one has yet been higher in Europe.) The shepherds, who served me as guides, all assured me that this fog had however passed over these mountains.

15. The lowest part of the fog was the thickest and driest. I assured myself of this by proceeding from the borders of the sea to the summits of the highest mountains.

16. It is probable, according to every account, that this fog overspread almost all Europe, the islands of the Mediterranean, and a part of Africa. It covered the whole Adriatic Sea, but extended only to the distance of 100 leagues on the ocean. It was properly a continental fog.

17. The thunder this year (1783) occasioned great devastation. In Provence and Dauphiny alone it killed nearly sixty persons; and a great number of animals. I have found no instance of its falling and doing mischief in places higher than 450 toises above the level of the sea. It would be too tedious to insert here the name of every place, which I noted down in my journal, where I learned that the thunder fell. It will be sufficient to relate the most singular effects it produced, and such as may add to our knowledge respecting the nature of these terrible meteors.

According to Pliny, Plutarch, Seneca, &c. the olive, fig-tree, and laurel, are never struck by lightning. Porta adopted this opinion, which was that of all the ancients. Toaldo says also, it is only those trees which contain resin that can perhaps escape the danger of thunder; such as the olive-tree, laurel, fir, and some others of the like kind. This, in all probability, is the foundation of the practice, common among the populace, of keeping in their houses, and placing on the summits of steeples and at the corners of fields, branches of olive that have been blessed, and of burning them in their houses during storms. It is possible, and even probable, that trees of this kind may be less frequently struck by lightning than those which contain a great deal of aqueous juices; but I can assert, that these preservatives of the ancients are not

equal to our conductors. On the 21st of June the lightning burnt the leaves of a fig-tree, and all the bark of an olive-tree, in the territory of Sallon; and I learned that the same thing had happened formerly, and that there even had been fig-trees and olive-trees split and tore to pieces by thunder.

There is some kind of thunder, says Seneca, accompanied with a loud report, by which men fall dead, and some become stunned and lose their senses. In 1783 I saw several instances which confirm the truth of this observation. At Pellissanne in particular, which is scarcely a league from Sallon, the thunder, attracted by an iron cross, killed two persons, and deprived several others, as it were, of their senses. The same thing happened at Freissinouse, in Dauphiny.

Thunder sometimes, says Seneca, renders wine frozen and congealed. Of the two persons killed by the thunder at Pellissanne, one lost immediately all his natural heat, and the body was found extremely cold. Thunder must sometimes produce these effects by occasioning a strong and speedy evaporation.

Bodies struck by thunder, says Plutarch, do not corrupt; dogs and birds do not eat them. This may sometimes be the case, but I know several examples of the contrary\*. On the day of the fair of Villefranche (July 22d) the thunder killed a mule, which corrupted so soon that it was found necessary to remove it. About eight or nine years ago, several sheep were killed by lightning on the mountain of Sederon in Provence; and the shepherds abstained, for a long time, from approaching the place, on account of the bad smell which they emitted. On the 1st of July, about four in the afternoon, the lightning fell at Carpentras on the Lazaret, (a place where those who died of the plague were formerly buried,) and set fire to the wood-work. The fire being communicated to several quintals of gunpowder, an explosion took place, by which five persons were killed and fifteen wounded. The same flash of lightning traversed the convent of the Carmelites, and melted part of the scissars of the prior: it killed also a cow, which was eaten without any bad consequence.

\* See Mr. Aebard's *Experiments*, in *Philosophical Magazine*, Vol. III. p. 51.

Towards the end of the month of June, the curé of Espinouse, in Provence, standing at the door of the church in order to exorcise the thunder, was killed, as well as his maid-servant and his clerk. At Mane, in Provence, the bell-ringer was killed by it. On the 26th of August the lightning was attracted by the iron cross on the church of Sigoger du Hai, in Dauphiny; and, going round it both on the inside and outside, frightened the ringer so much that he swore he would never return thither again in the time of a storm. At Pernes, in the Comtat, it overturned a cross. This cross, the remains of which I saw, was of white calcareous stone, and fastened by a bar of iron to a column of *coquilliere* stone\* of a yellowish colour. The iron had disappeared; a part of the column was shivered, and blocks of it carried to the distance of more than twenty-five paces.

At Aix the thunder dried up a beam, and left in one place only the fibrous part. At Fressinouse, in Dauphiny, it killed two oxen; deprived a labourer of his senses, and carried away one of his toe-nails. I must here remark, that his shoes were shod with iron. At La Motte du Caize, in Provence, it entered at a window, where there was no iron, and went out at the chimney. At Claret it unsaddled an ass without doing him any hurt, and carried the pack-saddle to a considerable distance. In several places, and particularly at Bannon in Provence, the lightning tore off the hair from the heads of several women. At Saint Cristol, in Provence, it carried away the half of a girl's body. At Avignon it carried away the half of a cat. I had this fact from M. Sauvan, who on that day (June 21st) observed the ball on the steeple of the Grands Augustins of Avignon covered with a crown of light, which continued three quarters of an hour, and disappeared at eleven at night.

On the 22d of July there fell a great quantity of hail at Saint Esprit: it was of a very large size. A girl, struck on the head by a grain of it, applied her hand to the place and found her head-dress on fire.

On the 21st of June ascending thunder was seen at Sallon:

\* A kind of stone mixed with abundance of shells.

like phenomena were observed the same day in several other places. In several villages through which I passed, I was shewn a great number of trees stripped of their bark by the thunder; and I remarked that the part of the bark, or wood, carried away, was almost always broad at the bottom of the tree, and narrow at the summit. It would seem as if the lightning had met with obstacles, and that it had not the same force when it attacked the higher parts of the tree. In my opinion the thunder which produced these effects was all ascending; and I often observed holes at the roots of the trees which had been deprived of their bark. I observed also that dry land had been less struck with the lightning than moist land, which induces me to believe that the greater part of the thunder that year was ascending.

## II. *Thoughts on the Origin of the Electric Fog.*

Several philosophers adopted the opinion of the populace, and considered this fog as a natural effect of the earthquake which laid waste Sicily and a part of Calabria. Toaldo thought that all these exhalations were brought from Calabria and Sicily by the winds which blew from the southward: but this respectable philosopher, at the time when he wrote, did not know that the fog was almost general throughout Europe. Besides, the earthquakes in Calabria and Sicily took place chiefly in February, and the fog did not appear till the middle of June; that is to say, till more than four months after. In my opinion, then, this fog was not occasioned by the earthquakes of Calabria and Sicily; but the fog and these earthquakes, as well as those which we are assured took place in Iceland, had a common cause, which produced different effects according as they were modified by circumstances and the nature of the places. Let us now search for this cause; we shall find it in the annals of meteorology, and nothing is necessary but to distinguish it.

I have said, in another place, that the constitution of the atmosphere depends chiefly on the nature and form of the ground, and that the revolutions of the air are subject to the

\* *Journal de Physique*, Mars 1782, p. 187.



revolutions of the earth, over which they have an influence in their turn. If we consider the different bodies or substances of which that part of the earth known to us is composed, we shall see that, notwithstanding their apparent rest; they all obey, as we may say, an intestinal motion; and that this motion gives continual rise to new compositions and combinations, the connection of which escapes us, but which nevertheless exist. The remains of animals and vegetables still distinguishable, and which occupy so much room in our globe; the acids which attack them; the æri-form fluids disengaged from them; the metals and pyrites brought to perfection, or decomposed; the fires separated, or collected; the fermentations and effervescences; in a word, the innumerable decompositions and recompositions of all the parts of the earth, furnish abundance of subtle matter, which, by its levity, disengages itself at the surface of the globe, cannot, in certain circumstances, resist the gravity of the air, and rises sometimes to the summit of the atmosphere. On the other hand, the rains which fall, moist fogs, and several other causes, make a part of the water, which detaches itself from the air, to penetrate the earth, and to combine with the substance of these exhalations. It is afterwards, in part, attracted by the external heat, or repelled by the heat of the earth itself; but it never returns to the atmosphere as pure as it was when it issued from it. It carries with it a great part of that subtle matter of which I have spoken; and it is this afterwards which produces thunder, and almost all fiery meteors. There is then a continual communication from the earth to the atmosphere, and from the atmosphere to the earth. The greater part of these exhalations, in general, is specifically heavier than the atmospheric air; and, if they are not impregnated with a certain quantity of water, they cannot quit the earth. They must necessarily be there collected, and accumulate to a greater or less depth in the time of great droughts.

Let us now consult our registers, and those of different observers, and we shall see that there had prevailed, at least for nine years, an extreme drought, not only in Europe, but also in Africa and America. This was announced year after

year

year by our domestic as well as foreign gazettes; and we may conclude so from the minute observations of the celebrated Van Swinden, and the tables published by Toaldo, Cotte, Beraud, Beguelin de Romily, &c. Sometimes, indeed, rain took place in one country or other; but in general, till the preceding winter, an extraordinary drought prevailed. It began about 1774, and in the month of June 1782 was extremely great in Italy and in our southern provinces. We experienced at that period a suffocating heat; the earth, as we may say, seemed to be on fire, and, in the Plain of Camargue, scorched the feet of the reapers to such a degree that they were obliged at length to walk upon straw: several died of heat with the sickles in their hands, and there were a great many sick. In a word, the drought and heat were so excessive, that, at two leagues from Sallon, the spiders, which in general are not venomous, occasioned by their bite violent diseases, which had a great affinity to those occasioned by the bite of the tarantula.

In consequence of this great drought, the exhalations of the earth, specifically heavier than the air, and deprived of that humidity which serves them as a vehicle, remained in the bosom of the earth, where they must have formed immense accumulations. The winter of 1782—1783 was rainy, particularly in Calabria and Sicily; and the Alps were covered with a great deal of snow. The spring also was in general rainy. The water then being filtered into the bowels of the earth, was at first absorbed by the very dry exhalations there confined. This humidity, added to the warmth of the spring, no doubt occasioned effervescences and fermentations; so that the exhalations, disengaging themselves with violence, in certain places convulsed the earth, as was the case in Calabria and Sicily. In proportion as the water filtered into the earth by its own weight, it found new exhalations, which, by disengaging themselves, occasioned new convulsions, but less considerable on account of the less abundance of these exhalations. In places where they were heated by their mixture, they liquefied stones, and threw up volcanic islands, as in Iceland. In the last place, these subtle exhalations rising into the atmosphere from all parts, with the vapours,  
which

which served them as a vehicle, did not at first alter its purity, being intimately connected with it; but they nevertheless existed, and produced in its highest regions the multiplied halos, parafelena and parhelia observed that year. The heat increasing, and the earth continuing to furnish exhalations in proportion to the preceding humidity, these were communicated to the atmosphere in a manner almost insensible: but the atmosphere being at length saturated, these exhalations underwent new decompositions; storms were formed, the atmosphere was cooled, and suffered to escape a part of these exhalations, which fell again towards the earth, and in one day Europe was covered with a dry fog\*. Local circumstances, in regard to moisture, winds, and clouds, exempted certain places from it for some time. The earth, however, continuing to furnish exhalations, and these being united to those which the atmosphere, as we may say, had deposited, descending and ascending thunder were seen till the exhalations of the earth and the atmosphere were consumed. The atmosphere being gradually purified, and the source of the exhalations exhausted, the earth ceased to be convulsed.

XVIII. *Report on the Conversion of Soft Iron into Cast Steel by means of the Diamond. Read in the French National Institute, Thermidor 26, Year 7. By C. GUYTON†.*

THE Class will recollect the account which I gave of the grand experiment of the combustion of the diamond in oxygen gas in the focus of the lens of Tschirnhausen, and the new facts which I thence deduced respecting the true nature of the diamond; plumbago, which is its oxyd in the first degree; carbon, which is its oxyd in the second degree; and the carbonic acid, which is the produce of its complete oxy-

\* The same fog, I presume, took place in America, where there had been great complaint of drought for eight years. It was not seen in the open sea, because it was absorbed by the water: for this reason it did not appear in countries where the sky was overspread with clouds.

† From the *Annales de Chimie*, No. 92.

genation. They suggested to our brother, C. Clouet; the idea of searching for a confirmation of a new kind, by trying to make soft iron pass to the state of steel by cementation with the diamond.

It has hitherto been considered as certain, that iron does not melt but by passing to the state of steel or cast iron. But in what state does the carbon enter into that combination? It might be conjectured, that it is in the state of plumbago, or oxyd of the first degree; since that which is separated by acids exhibits the brilliant blackness and incombustibility which form its principal characters. Hence some were inclined to conclude, that the carbon entered into this union in the state of an oxydule; that consequently the carbon employed in the cementation of steel began by deoxydating itself to a certain degree. This was even in some measure proved; as the carbon employed for this operation was indeed found to have a more brilliant aspect, and nearly resisted incineration, like carbon in a mass burnt in close vessels. But if carbon really burns in the cementation of iron, it ought to disengage from it oxygen gas. This is a question which I have endeavoured to resolve by experiment.

I cemented small bits of iron in a porcelain retort, which in the preceding operation had received a vitreous coating, and which consequently was no longer permeable to air. These fragments were all surrounded, on every side, by charcoal of beech pulverised, and very dry. The retort was put into the reverberating furnace, and a tube connected to it and carried under a receiver filled with mercury. There was disengaged a quantity of elastic fluid, composed of carbonated hydrogenous gas and carbonic acid gas, the last of which was at first only 0.11 in bulk; towards the middle of the experiment, 0.13; and at the end, 0.15.

The conversion of iron into steel being found only little advanced, after three hours and a half exposure to the fire, we put the same iron and the same carbon again into the retort, and exposed it to the heat of a three-blast-furnace. This time there was only a very small quantity of gas; but it was still carbonated hydrogenous gas mixed with carbonic acid gas, and always with the same progression of the latter; which

which made at first only 0.07 of volume, while the last portions contained 0.12. The iron on this occasion was converted into steel, and even the fragments had united by a commencement of fusion.

It was very probable that a part of the carbonic acid, collected in this operation, might have been formed at the expense of the remaining carbon and with disengaged oxygen; but the constant presence of the hydrogen only served to indicate the difficulty of freeing the carbon entirely from the last portion of water it contained. I shall here take occasion to observe, that this experiment seems not at all reconcilable with the opinion of some chemists, that hydrogen has more affinity than carbon for oxygen: an opinion which they found on this circumstance, that carbon is precipitated, in Volta's eudiometer, when a mixture of oxygen gas and carbonated hydrogen gas is made to detonate; if a quantity of oxygen sufficient to acidify the two bases has not been employed. I say, that this affinity was not exerted in my experiment: for it cannot be doubted that the temperature was high enough to reproduce water by the union of the oxygen and hydrogen; and we can here see nothing which could decide a preference of the oxygen for the carbon.

These considerations seemed to me sufficient to create a new interest in regard to the experiment proposed by C. Clouet. I did not hesitate, therefore, to employ in it one of the diamonds preserved in the cabinet of the Polytechnic school, according to the leave granted by the Council; being persuaded that if it disappeared in the operation, merely by exposure to a high temperature, in contact with iron, without the accession of the air or any other oxygenating substance, the fact thereby established would leave no room to regret having sacrificed it.

Citizen Clouet had himself prepared a small crucible of soft iron, forged on purpose out of picked heads of nails. Its form was a solid of eight planes. (Plate II. fig. 3). It was shut by a stopper of the same iron well adjusted. (Fig. 4.)

This crucible was to be placed in a Hessian crucible, furnished with a cover well luted. This was all the apparatus for the experiment. I cannot give a better idea of the

result than by the report drawn up by C. Clouet, Welter, and Hachette.

*Report of the Experiment made at the Polytechnic School, Thermidor 25, Year 7, respecting the Conversion of Iron into Steel by the Diamond.*

"The diamond employed weighed 907 milligrammes. As it did not entirely occupy the crucible, we filled it with filings of the same iron as that of which it was formed. The crucible was shut by its iron stopper, which was forcibly thrust home, that as little air as possible might remain in the inside.

	Grammes.
"The crucible and stopper weighed together	55.8
"The iron filings which covered the diamond	2
	<hr/>
"Total weight of the iron surrounding the diamond	57.8

"After having cut off the excess of the stopper\*, the crucible was placed alone, and without the addition of any surrounding matter, in a very small Hessian crucible, and the latter in a second crucible of the same earth; but the space between the two latter crucibles was filled with siliceous sand free from all ferruginous particles. In the last place, the large crucible was luted with earth arising from pounded crucibles and unbaked clay, and the whole was exposed about an hour to a three-blast-forge fire.

"The whole being cooled, we found, in the interior Hessian crucible, the iron converted into an ingot of cast steel. (See fig. 5.) It formed, with the stopper and filings, but one round mass well terminated, some few globules excepted, which were detached, and which weighed only 884 milligrammes.

	Grammes.
"The ingot of cast steel weighed	55.500
"The detached globules	0.884
	<hr/>
"Total weight of the steel obtained	56.384

\* This portion of the stopper, as well as the remainder of the ingot of which the crucible was formed, were subjected to the Class for inspection, in order to ascertain the nature of the iron employed.

"The

“The iron and the diamond weighed, before the operation, 58.707 grammes; from which it follows that there was a loss of iron about 2.423 grammes. This iron had given to the Hessian crucible the colour of plumbago.

“(Signed) CLOUET, WELTER, HACHETTE.”

The fusion of the iron being so far perfect as to shew on its surface the rudiments of the most beautiful crystallisation, it is not possible to think that any part of the diamond could have remained in the inside untouched, or that it was not in the most intimate state of combination. The difference of the specific gravity opposes such an idea\*.

Thus the diamond disappeared by the affinity which iron exercised on it by the help of the high temperature to which they were both exposed, in the same manner as a metal disappears in the alloy of another metal.

The diamond, therefore, has furnished here the same principle as carbon, since the product of the union has the same properties.

The conversion into steel is not doubtful. The ingot having been polished on a lapidary's wheel, a drop of weak nitrous acid immediately produced a dark-grey spot, absolutely like that exhibited on English cast steel, and on cast steel produced by the process of C. Clouet. Those who have often tried steel by this kind of proof, long ago pointed out by Rinmann, had occasion to remark, that the spot of cast steel, though very sensible, is however less black than that of steel made by cementation, which depends perhaps on the different degree of oxydation of the carbon which they have taken in.

#### *Explanation of the FIGURES.*

A, fig. 3, (Plate II.) is the plan of the iron crucible. B, a section of this crucible. C, fig. 4, the stopper of the crucible. D, E, fig. 5, ingot of cast steel seen in perspective. The spot formed by the nitric acid on the polished part is represented at *a*.

\* Some persons having expressed a desire to see the inside of the ingot, it was broken on the anvil, which was not effected without several blows from a very large hammer. It divided itself into two fragments, which were exhibited at the next sitting. The fracture appeared perfectly uniform, and of the most beautiful grain.

XIX. *Ninth Communication from Dr. THORNTON, Physician to the General Dispensary, relative to Pneumatic Medicine.*

A DISEASED LIVER CURED BY VITAL AIR:

THE butler of Colonel Ironside, who had been long resident in India, laboured for several years under a well-marked liver complaint. He had been under the care of Dr. Warren, and other physicians, without experiencing any essential advantage. Colonel Ironside, as all other means had been ineffectual, wished him to try the vital air. He accordingly came under my care, and the same tonics were employed as he had before taken; therefore I ascribe the cure to the vital air, which was conjoined with these, and which soon completely restored him to health; and he has continued well now above a twelvemonth.

*Observations.*

When animals placed in pure vital air were destroyed by so powerful a stimulus, Dr. Beddoes found the liver not liver-coloured, but of a florid red. We therefore can easily suppose it to act on this organ when given in a moderate way: and as the oxyds of mercury and the nitrous acid, which are the best remedies for this disease, act chiefly from their contained oxygen, it is probable that the vital air will hereafter be found a specific in this complaint, possessing superior advantages over both these remedies, and will supersede them, although it cannot be put up into two-ounce phials, the principal objection raised against it; for the airs can now (which originated from my suggestion) be confined in barrels, and bottled off as easily as wine: and I must observe, that this patient had a barrel of vital air, containing 24 gallons, which cost him one guinea, conveyed for him to his master's seat in the country, and a tin pneumatic apparatus for inhaling the medicinal air, which stood him at the low rate of thirty shillings; which improvements, I trust, will greatly facilitate the general application of pneumatic medicine, when it will be sure to find that level its merits entitle it to.



## INTELLIGENCE,

AND

## MISCELLANEOUS ARTICLES.

## ELECTORAL ACADEMY OF SCIENCES AT MANHEIM.

ON the 16th of April last the Electoral Academy of Sciences at Mannheim held a public sitting in commemoration of its deceased founder the Elector Charles Theodore, who died on the 17th of February. On this occasion Mr. M. Collini, a member of the Society, read a printed oration on the vicissitudes of the Academy. The most flourishing period of this institution was that between the time of its establishment and the year 1777, when the Palatinate family got possession of Bavaria, and Charles Theodore transferred his residence from Mannheim to Munich. It was not, however, left destitute of support; but its distance from the electoral court rendered many things impossible, which, under other circumstances, might have been accomplished. Still more prejudicial to the Academy, as well as the whole Palatinate, was the war which broke out afterwards, between France on the one side, and the German Empire and Austria on the other. Mannheim was bombarded three times, *viz.* in December 1794, November 1795, and January 1798; and as often was it necessary to remove the library of the Academy, as well as its papers, &c. to a place of safety, and to arrange them. The sittings and labours of the members were interrupted; the palace, assigned to them for holding their meetings, was in part burnt; and as the revenues destined for the support of the institution arise from possessions on the left bank of the Rhine, if these remain in the hands of the French, it is to be apprehended, that, like many other establishments of the same kind on the right bank of that river, which had their revenues on the other side, it will decline unless supported by the new regent. Among its most distin-

guished members, since the time of its foundation, may be reckoned Voltaire, elected in 1764; Lessing, and Lalande the celebrated French astronomer. It was established in 1763 according to a plan of the learned Schopffin, and divided into two classes, the Historical and Physical; the latter of which, in the year 1780, was subdivided into the Physical (properly so called) and Meteorological. The collection of the papers of the Academy have been published in eleven volumes quarto, under the title of *Acta Academiæ Theodoro-Palatinae*. The Meteorological Observations are from 1781 to 1792, and make twelve volumes quarto, with the title of *Ephemerides Societatis Meteorologicae Palatinae*.

#### SUGAR FROM BEET-ROOTS.

Professor Klaproth has published the following testimony in regard to sugar procured from beet-roots:

"Having, in consequence of a request by Mr. Achard, that I would examine his process for making sugar from beet-roots, and communicate to him the result, convinced myself, by repeated experiments, of the abundance of saccharine matter in these roots, that I might ascertain the proportion with more accuracy, I made the following experiment:—Twenty-five fresh roots, which weighed  $32\frac{1}{2}$  pounds after they had been scraped and the tops cut off, were put into a perforated tin vessel, and the juice was expressed by a proper apparatus. The juice obtained weighed  $19\frac{1}{2}$  pounds. The squeezed residuum was put into a tin kettle with boiling water, and, after standing an hour, was pressed also. This extract was added to the juice; both were boiled to a syrup, and strained through a woollen cloth; after which the syrup was put into a porcelain capsule, and evaporated gradually over a slow fire to complete dryness. The raw sugar obtained, and which I put into a sealed glass, amounted to two pounds twelve ounces.

"That 25 roots, or  $32\frac{1}{2}$  pounds of beets, produced two pounds twelve ounces of sugar, is hereby certified, as witness my hand,"

MARTIN HENRY KLAPROTH."

"Berlin, 11th January 1799."

A letter

A letter from Berlin, dated September 28, states, that the commission appointed by his Prussian majesty, to examine Achiard and Klaproth's process for making sugar from beet-roots, have finished their report on that subject: The result is, that 1500 pounds of beet-root gave 398 pounds of very agreeable syrup, which produced 57 pounds of powder-sugar of a white colour, and proper for use without being any farther refined.

#### INDIGO RESIN.

Professor Brugnatelli, of Pavia, has lately announced, that, by treating indigo with the nitrous acid, one may obtain a large quantity of a peculiar resin, which he calls *Resina indigofera*. An ounce of indigo distilled with four ounces of the nitrous acid, left a thick mass, in which he found a concrete substance of a red colour, which, when separated, had all the properties of resin: it weighed half an ounce. This resin dissolved very easily in alcohol, which acquired from it a dark red colour. Water decomposed this solution immediately, and produced a yellow precipitate. This tincture communicated its colour to paper and linen, and gave it a beautiful and durable yellow dye. It communicated the same colour to the skin and the nails: the former retained its colour for a long time. This resin has a very bitter, astringent, and disagreeable taste; but no sensible smell. On glowing coals it melts and burns with a flame, and at the same time emits a sharp disagreeable smell.

#### CONVERSION OF IRON INTO STEEL.

Our valued correspondent Mr. Mushet, of the Clyde Iron-Works, has examined and repeated C. Clouet's process with considerable success. The experience of Mr. Mushet in similar processes rendered him well qualified for the investigation, and the result has been what we should have promised from our knowledge of his abilities and accuracy. Mr. Mushet, instead of experiencing any loss in the weight of iron employed, has uniformly obtained the same increase gained in the large way of manufacture by the common process of cementation with charcoal, *viz.* from  $\frac{1}{3}$  to  $\frac{1}{2}$  th. However, in varying the experiments of the French chemists, he has

met with phenomena that force him to draw very different conclusions from them, and to reject the idea of the carbon being furnished by the decomposition of the carbonic acid; for, in some experiments, he employed for the cementation calcareous earth, previously deprived of its acid, and excluded the contact of external air, and the result was cast steel. These experiments were performed in crucibles made of Sturbridge-clay, without any mixture. Superior effects, however, were produced in half the time, by presenting carbon, in a comparatively deoxydated state, in crucibles variously compounded. Some curious questions naturally arise from the result of these experiments; especially from the first, as, Whence comes the carbon? But as Mr. Musket has promised, in some future Number, to lay his experiments on this interesting subject before the public, our philosophical readers will then be better enabled to enter upon the investigation.

#### IMPROVED WRITING INK.

C. Van Mons has applied the discoveries of Proust to the preparation of common writing ink. He has found that the sulphat of iron, calcined to whiteness, always gives a most beautiful black precipitate. By the following mixture he obtained excellent ink:—Galls, 4 ozs.; sulphat of iron, calcined to whiteness,  $2\frac{1}{2}$  ozs.; and two pints of water. The whole must be left to infuse cold for 24 hours; then add gum Arabic 10 drachms, and preserve it in a stone jar, either open, or covered merely with paper.

#### MANGANESE IN VEGETABLES.

M. Proust has lately published the following short way of proving the presence of manganese in such vegetable productions as contain any; which has this to recommend it, that it is shorter and more effectual than that of Scheele. Put in the process distilled vinegar (employing heat) on well-washed ashes, without attempting, however, to extract from them all that they are capable of yielding to the acid. The liquor then contains oxyd of manganese, lime, and magnesia. Try it by the prussiat of pot-ash, and it gives a precipitate of the colour of peach blossoms, which, treated by the blow-pipe in the usual manner,

manner, gives constantly the colour which denotes the presence of that oxyd. This precipitate is not without a little iron, which it receives, as I believe, from the prussiat of potash. Then put nitric acid over the residuum of the ashes, and it will take from them the iron, which may be proved also by the prussiat. Then search for the different earths, which may be discovered by the common means. The ashes of the pine, calendula, vine, green oak, and fig-tree, contain manganese. The ashes of the fig-tree are almost all siliceous; the ashes of barilla do not contain an atom of it, but iron in great quantity, magnesia, &c.

#### PREPARATION OF STUFFS FOR DYEING.

A letter from Van Mons to Brugnatelli, printed in the 92d number of the *Annales de Chimie*, contains a useful hint on this subject. He expresses himself as follows:

“The memoir, read by Giobert to the Academy of Turin on the animalisation of flax and cotton, induces me to mention a circumstance which ought to have led me to the discovery of this animalisation long ago, had I paid proper attention to it at the time. I had caused some oyster-shells to be boiled with pot-ash and lime, in order to whiten them for some pharmaceutical purposes; and I employed the ley produced by that operation, to bleach the linen cloths used to strain the decoctions. My pupils made me afterwards observe, that these pieces of cloth were strongly dyed when any substances were passed through them, the colour in which might have been carried off by merely washing in cold water.”

#### YELLOW DYE FROM A SPECIES OF MUSHROOM.

Among the different kinds of mushrooms, capable of producing lively and durable dyes, none is more worthy of notice than the *Boletus birsutus* of Bulliard, from which C. Lasteurie has extracted a bright, shining, and very durable yellow dye. This pretty large mushroom grows commonly on walnut and apple-trees. Its colouring-matter is contained in abundance, not only in the tubular part, but also in the parenchyma of the body of the mushroom. In order

to extract it the mushroom is pounded in a mortar, and the liquor thence obtained is boiled for a quarter of an hour in water. An ounce of liquor is sufficient to communicate colouring-matter to six pounds of water. When the liquor has been strained, the stuff to be dyed is put into it, and boiled for a quarter of an hour. All kinds of stuff receive this colour and retain it, but on linen and cotton it is less bright. This colour may be modified, in a very agreeable manner, by the effect of mordants.

The process succeeded best on silk. When this substance, after being dyed, is made to pass through a bath of soft soap, it acquires a shining golden-yellow colour, which has a perfect resemblance to the yellow of that silk employed to imitate embroidery in gold, and which has hitherto been brought from China and sold at a dear rate, as the method of dyeing it is unknown in Europe. The yellow colour extracted from this mushroom may be employed also with advantage for painting in water-colours as well as in oil.

#### NATURAL HISTORY.

The two following articles on the Friendship and the Sagacity of Birds, by Mr. Simpson of New-York, we copy from the Medical Repository, a valuable work published in America:—

“ Mr. Myers, a brother-in-law of mine, moving from Wilton to Philadelphia, desired me to send for a large turkey-cock and hen, and a pair of bantams, which had been a long time in his yard, and which the family did not choose to have killed. Accordingly, after his departure, I had them brought home, and put with some other poultry that were then running in my yard. Some time after, as I was feeding the poultry from the barn-door, a large hawk turned the barn, and suddenly made a pitch at the bantam hen: she immediately gave the alarm, by a noise which they generally make on such occasions; when the large turkey-cock, who was about two yards distance, and who I presumed saw the hawk's intentions, and the imminent danger of his old acquaintance, flew at the hawk with such violence, and gave him such a severe stroke with his spurs as he was going to seize

seize his prey, as to knock him from the hen to a considerable distance; and the timely aid of this faithful auxiliary, the turkey-cock, saved the bantam from being devoured by the hawk."

During my residence at Wilton, early one morning I heard a noise from a couple of martins, who were jumping from tree to tree adjoining my dwelling. They made several attempts to get in a box or cage fixed against the house, which they had before occupied; but they always appeared to fly from it with the greatest dread, and repeated those loud cries which first drew my attention. Curiosity led me to watch their motions. After some time a small wren came from the box or cage, and perched on a tree near it, when her shrill notes seemed to amaze her antagonists. After some time she flew away. The martins took this opportunity of returning to their cage: but their stay was short. Their diminutive adversary returned, and made them fly with the greatest precipitation. They continued manœuvring in this way the whole day, and I believe the wren kept possession during the night. The following morning, on the wren's quitting the cage, the martins immediately returned, took possession of their mansion, broke up their own nest, which consisted of twigs of different sizes; went to work, and with more industry and ingenuity than I supposed they possessed, they soon barricaded their doors. The wren returned, but could not re-enter. She made attempts to storm the works, but did not succeed.—I will not presume to say that the martins followed our modern maxim, and carried with them a sufficiency of food to sustain a siege, or that they made use of the abstinence which necessity sometimes, during a long and bad storm, might probably occasion; but they persevered for near two days, to defend the entrance within the barricado; and the wren, finding she could not force an entry, raised the siege, quitted her intentions, and left the martins in quiet possession without further molestation.

#### DEATHS.

On Friday, the 11th of October, Samuel More, Esq. who for many years past filled the office of Secretary to the Society

for the Encouragement of Arts, Manufactures, and Commerce. The assiduity and ability with which he discharged the duties of the office he held, for that respectable Institution, will make it difficult to find another to fill it who may be as well qualified.

A few weeks ago, at New-York, Dr. Perkins, inventor of the metallic tractors. He had gone thither, from his usual place of residence in Connecticut, to assist his brother practitioners in endeavouring to arrest the progress of the yellow fever, with which that city has been again visited, and caught the infection, which carried him off.

In the Philosophical Magazine for April last we announced the death of that celebrated naturalist Spallanzani on the 11th of February. It appears, however, that he died on the 17th; and, as every thing that relates to the last moments of eminent men is interesting, we hope our readers will not be displeased with us for adding the following particulars:—On the evening of the 10th he was seized with an apoplectic fit, the consequence of a neglected strangury. Professors Brera and Scarpa, who were called in to visit him, immediately saw that all medical aid would be useless; for, on inspecting his urine, it was found that the bladder was in a state of gangrene. They however ordered him such medicines and treatment as they thought could be of any service. On the 12th the affection in the left arm and the lips disappeared. On the 13th he recovered the full use of his senses; found himself much better; and, without apprehending death, attended to his literary and domestic concerns. Early on the morning of the 17th he was attacked by a general debility, accompanied with a diarrhoea entirely colliquative. The two professors then told him, that this day would be the last of his life; an information which Spallanzani received with the greatest firmness of mind. Towards noon he embraced his friends, the two professors, and particularly his brother, who had come express to Pavia from Scandiano, in the duchy of Modena; took the most affectionate leave of them, and exactly at 11 o'clock breathed his last in their arms. The opinion of the physicians respecting his disease was confirmed by the opening of his body. A history of it has been published by

Professor



Professor Brera, who was two years physician to this eminent naturalist. Two monuments are to be erected at the public expence to his memory; one in the Pantheon of Milan, and another within the precincts of the University of Pavia.

In the same Number of the Philosophical Magazine we announced also the death of Professor Lichtenberg, of Gottingen, on the 24th of February. Since that period a small work has appeared at Gottingen under the title of, *Elogium Georgii Christophori Lichtenberg, in concessu Soc. Reg. Scientiarum recitavit A. G. Kastner, 20mo. d. Ap. 1799*; from which we extract the following particulars:—Professor Lichtenberg was born on the 1st of July 1744, at Oberamstadt, in Hesse Darmstadt. He was of a weakly constitution from his birth, and applied himself very early to the mathematics and natural history, in which he was greatly assisted by his two brothers, who were older. In the year 1763 he went to Gottingen, where he became the pupil and friend of Kastner and Meister, and attended the Lectures of the other professors. In conjunction with Erxleben he made some observations on the earthquake of 1767; and remarked, in particular, that the shock continued six seconds; while others, not accustomed to mathematical accuracy, extended its duration to a whole minute. He delineated the courses of the comets which had been observed; and it is remarked, that he possessed a great deal of patience for works of this kind. Professor Kastner has now in his possession a drawing by him, of the face of the moon, where the spots are marked in the order in which they enter the earth's shadow during eclipses. In the year 1770 he was invited to Gießen; but he preferred Gottingen, where he was appointed Professor Extraordinary. In the years 1772 and 1773 he was commissioned to determine, from astronomical observations, the position of various places in his Britannic Majesty's German States; and for this purpose was supplied, at the king's expence, with a quadrant by Sisson. In 1776 he laid the result of this labour before the Royal Society, of which he had been elected a member two years before. As repeated wishes had been expressed that Mayer's manuscripts might be published, Professor Kastner put them into the hands of Lichtenberg, who, assisted by Dietrich, published

published the first volume, in 1775, with a typographic elegance which had been before unknown in Germany. Want of leisure prevented him from editing a second. Lichtenberg was twice in England; the first time in 1770, and again in 1774. In the year 1777, after Erxleben's death, he studied natural philosophy, in the introduction to that science written by his predecessor; the four last editions of which were published under his inspection, but always with additions by himself. On this occasion he made a collection of philosophical instruments, which afterwards became the property of the University. Among the new experiments, which were peculiarly Lichtenberg's own, is that respecting the figures formed by pounded resin on the electrophore, by which he examined and traced out the nature and progress of the electric fluid in a new manner, and communicated the result of his researches in two papers, read before the Royal Society in 1777 and 1778. Professor Lichtenberg had a very extensive correspondence, which enabled him to communicate to the Society a variety of useful intelligence. He proposed, alternately with Kastner, the prize questions of the mathematical class, and was the author of the last for the present year, respecting the motion of steam in conductors of a given kind. In the year 1780 he began, in conjunction with the late J. R. Forster, the Gottingen Magazine, six numbers of which appeared annually, till 1785, when it was dropped with the first number of that year. In the Gottingen Pocket Calendar, of which he was the editor for many years, he published various articles equally useful and agreeable. Among these was an account of Hogarth's satyrical prints, accompanied with engravings. The large account of these prints, with a commentary, is well known. As Professor Lichtenberg's complaints returned regularly every year, the last attack was at first not considered dangerous; but an inflammation of the lungs taking place, he was carried off on the 24th of February, as before stated.

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THE

PHILOSOPHICAL MAGAZINE.

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NOVEMBER 1799.

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I. *Observations on the Flux and Reflux of the Atmosphere.*  
*By the Abbé MANN\*.*

AS the general power of attraction or gravity extends to all bodies within the sphere of its action; and as the atmosphere of our earth consists not only of heavy, but also of moveable and elastic parts, which revolve around a common axis like the water of the ocean, it necessarily follows that the atmosphere must be affected by the same physical causes which produce the flux and reflux of the sea. This consequence is so immediate and necessary that it is not possible to doubt it: the question therefore will not be the existence of a flux and reflux, but merely respecting their extent. Many philosophers have asserted, on grounds which do not appear to have much validity, that the flux and reflux of the atmosphere amount to no more than those of the ocean, that is, eight feet. According to their opinion, a sea of water, air, or quicksilver, would be raised to about the same height by the effects of the sun and moon, and acquire from them nearly the same movement. Those who speak in this manner, must entirely overlook the nature of elastic fluids; for it may easily be conceived that, with the same power of at-

\* From the *Transactions of the Academy of Sciences at Brussels*, Vol. IV.

traction, the movement of an elastic fluid would be totally different from that of a non-elastic. Other philosophers, not neglecting this consideration, have believed that the causes which produce the movement of the waters of the ocean can occasion a much greater in the aerial mass of the atmosphere, and must have a very great share in producing the regular winds, and other phenomena which depend on them.

D'Alembert has calculated, from the theory of general gravitation, those movements which must be produced in the atmosphere by the effects of the sun and moon. He found that from this influence a continual east-wind must arise at the equator, and that, in the temperate zones, not far from the tropics, it must be converted into a west-wind; that on account, however, of many local circumstances, and other impediments which come in the way, it cannot always continue in the same direction; and that the variations in the state of the barometer, which thence arise, must be very inconsiderable, and almost imperceptible.

Bacon, Gassendi, Deschales, Goad, Dampier, Halley, and others who have written on the wind, all unanimously observe, that the periods of the year most exposed to it are the two equinoxes: that storms are most frequent at the times of new and full moon, and particularly those which happen near the equinoxes; that at periods, otherwise calm, a small breeze always takes place at high water; and that a small movement in the atmosphere is each time perceived a little after noon and midnight. Now, as most of these circumstances have a great resemblance to those which accompany the flux and reflux of the sea, and take place at the same time; and as in this respect the movement of water and the atmosphere is the same, according to Newton's rule, that natural effects of the like kind must be produced by the same causes\*, both may be deduced from the same source.

From this it appears that theory and experience coincide to establish the existence of an atmospheric flux and reflux, and of the regular and very perceptible effects which they produce. Nothing therefore is necessary but to deduce their

\* *Effectuum naturalium ejusdem generis eadem assignandæ sunt causæ.*

phenomena and extent from the well-known properties of elastic fluids. For this purpose we must have before our eyes the following principles of aërometry, to be found in most elementary books:—

1. The elasticity of fluids is in the inverse ratio of their density, and in the direct ratio of their rarity. Thus air is more elastic than water; light more elastic than air; and ether than light. This is one of the principles of Newton\*.

2. The force or elasticity of the air expands and contracts itself in the direct ratio of the weight with which it is loaded, and diffuses itself in the inverse ratio of the force by which it is compressed†.

3. The air is rarefied, or diffuses itself, in the direct ratio of the quantity of heat which acts upon it‡.

4. The air, as well as all fluids in general, has a tendency to put itself in equilibrium, and does not rest until it has obtained it§.

It now follows, from these principles, that every thing which increases the weight of the atmosphere, and presses the air more in one place than another near it, must occasion the air to rush from the former to the latter, where it is lighter: and, on the other hand, every thing that lessens the gravity of the atmosphere, and increases or rarefies the air, and makes it lighter than in the neighbouring parts, must occasion the air to rush in from all sides to those parts; and this continues in each case till the equilibrium is restored, and rest again effected. This is the principal cause of the origin of wind. But, as a pendulum put in motion does not immediately rest when it has come to a perpendicular direction, but obtains rest after making some small vibrations; in the same manner, the currents of air, from the places where the air is more pressed to that where it is less so, will rush beyond the boundaries of the equilibrium, from which they will again fly back, and at length obtain rest after several undulations. As this takes place in all fluids, it is more

\* Princip. in fine, p. 530, edit. 1726, et quæst. 21, 22, in fine optices.

† Wolf Elem. Aërom. § 72—77.

‡ Ibid. § 146.

§ Ibid.

§ 36—44.

peculiarly common to elastic fluids; and it is observed, according to these principles, that the winds during storms change their course, and blow exactly from those points to which the current was before directed.

The specific gravity of the air is 800 times less than that of water, and its elasticity is infinitely greater. The attractive power of the sun and moon lessens the gravity and pressure of the atmosphere towards the earth, as it draws it towards these bodies in the same manner as it does the water of the ocean. But this power of attraction does not extend the water of the ocean, which is destitute of elasticity, as it does the whole mass of the atmosphere (possessed of it, and exposed to the action of this power) in the ratio of the decrease of gravity, and of the pressure of the atmosphere towards the earth. Adopting, therefore, the inadmissible supposition, that water, air, and quicksilver would be raised to nearly the same height, and be moved in the same manner by the attractive force of the sun and moon; this much at any rate is proved, that the extension of the air, in proportion to the lessening of the gravity of the atmosphere towards the earth, must always take place, and produce a considerable atmospheric flux, while this effect cannot occur in regard to the flux of the ocean. It is certain that fluids are more or less subject to the effects of any power acting upon them, according as they have more or less mobility, which is in the direct ratio of their rarity and elasticity. Now, as air possesses these properties in a degree 800 times greater than water, the flux of the atmosphere must very perceptibly exceed in magnitude that of water. This, therefore, is a second certain argument against those who pretend that the flux and reflux of the atmosphere are similar to those of the ocean. Not only must the attractive power of the sun and moon, on account of the greater elasticity and mobility of the air compared with water, produce a greater effect on the atmosphere than on the ocean, but the component parts of the atmosphere, as they are nearer the moon by about a ninetieth part of the semidiameter of the earth, must be more strongly attracted than the watery particles of the ocean; in the same manner as those which are 90 degrees distant

distant from the place where the moon is vertical, must occasion a greater pressure on the earth on account of the greater obliquity of the attraction, and this in proportion to the greater height of the atmosphere above that of the sea. This is the third physical cause which makes the flux and reflux of the atmosphere more considerable than that of the ocean. To this we may add the immense space occupied by the atmosphere in comparison of the ocean; for if the whole globe were covered with water, this general ocean would not occupy the fiftieth part of the space actually filled by the atmosphere; and in my opinion the extension effected in elastic fluids, by the same power of attraction, is in proportion to their masses. Supposing this to be the case, the flux of air in an atmosphere of ten miles height, would be much more considerable than in another amounting to only a tenth part of that height: though this difference cannot take place in the flux and reflux of the ocean, because water is destitute of elasticity. Besides, lands, islands, straits, bays, the situation of the coasts, sand-banks, shoals, &c. throw a great many impediments in the way of the water in regard to its motion; whereas the atmosphere, which rises to a height ten or twelve times greater than the summits of the highest mountains, is in no manner prevented from moving according to the effects of the power of attraction, unless something very particular takes place in its lowest strata. It is more than probable that each of these causes contributes its part to make the movement of the atmosphere far more considerable than that of the ocean.

It is therefore certain that the united effects of the attraction of the sun and moon on the atmosphere of the earth must raise and extend it in the inverse ratio of the square of the distance so as to make it assume the form of a lengthened spheroid, the greater diameter of which, from the same grounds and with the same variations as the aqueous spheroid, must nearly follow the direction of the line of attraction.

Besides, the heat of the sun, which has no sensible influence in raising or extending the water of the ocean, will produce an effect on that part of the atmosphere exposed to its

rays

rays by heating and rarefying it, in the ratio of its strength, according to the third of the before-mentioned principles. That part, therefore, of the atmosphere which is gradually turned towards the sun, must extend in proportion to the degree of heat which acts upon it; and rise above the rest; and this atmospheric swelling will constantly follow the apparent daily course of the sun. The moon, on the other hand, has no power to produce or change this phenomenon; because, in all the experiments hitherto made to collect and concentrate its rays, the smallest degree of heat has never been perceived.

As there are therefore two different causes, *viz.* the united attraction of the sun and moon, and the heat of the sun alone; by which the latter, except at the time of the syzgies, when they both act in the same direction upon one point, has a particular influence on the atmosphere of the earth independent on that of the former, it thence follows that they will produce three different fluxes every day. Two of them arise from the attractive power of the sun and moon, and in their formation, direction, and movement, are perfectly similar to those produced in the ocean from the same causes. The third, however, is produced by the heat of the sun alone; and its prominent parts will always be in that parallel through which the sun passes in the course of his daily movement, and will continually follow that luminary, from parallel to parallel, at a small distance. The two first I shall call *attraction-tides*, the third *heat-tides*.

The tides of attraction, like those of the ocean, and from the like grounds, have at the same time, at two opposite ends of the globe, projecting parts, and these lie almost in that line which might be drawn from the centre of the earth to that of the moon. The heat-tides, on the other hand, can take place only on one point of the globe; that is, in the point to which the sun is vertical. Their projecting part will be directed towards that luminary, and nearly follows its movement.

In regard to the effects of these atmospheric tides, they depend on the natural state of the aerial fluid, and on its rest, which consists in an equilibrium of all its parts. When  
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this equilibrium is destroyed, the atmosphere returns to its former state as soon as the cause of this derangement is removed. If this equilibrium is by any cause destroyed in any one place, a movement must naturally follow from all sides, and continue until the equilibrium is again restored; and in this we may discover the principal cause of the origin of wind. Every thing, therefore, which can derange the equilibrium of the atmosphere in any manner, must be reckoned among those causes which give rise to wind: such as the flux and reflux of the atmosphere, occasioned by attraction and heat; all other rarefaction of the air by heat; or the condensation of it by cold, &c.; the letting loose of vapours by fermentation and evaporation into the atmosphere; the eruption from the bosom of the earth and ascent of elastic vapours, volcanoes, earthquakes, and perhaps other causes not so evident, and therefore less known. The exceedingly variable directions of the wind depend on the nature of the countries from which they blow; and, in particular, on mountains, forests, rivers, marshes, lakes. In a word, every thing that can oppose or favour the free movement of the aerial fluid has an influence on the direction of the wind.

Among all those causes which derange the equilibrium of the atmosphere, and contribute to the production of wind, the principal and most uniform is the rarefaction and condensation of the air. Both are the immediate effect of the different atmospheric tides. The regular course of these tides from east to west, in consequence of that of the sun and moon, must produce a continual east wind; which is however exceedingly weak, but continues without interruption, and prevails only in the immense seas of the Torrid zone, though with most regularity in the Pacific ocean, on account of its vast extent. The compressed parts of the atmosphere in the neighbourhood rush towards those most rarefied by the atmospheric tides, and follow them also in their regular progress from east to west. The parts of the atmosphere, however, in the west, will have a much less, or perhaps no movement towards those which proceed from the east, on account of the atmospheric flux moving towards them: at any rate, the motion from east to west will exceed that small motion

from west to east, and therefore produce a continual east wind in the parallel of the atmospheric tide. A wind also will proceed from the north or south of this parallel, in an oblique direction north and south, to the distance of about thirty degrees on each side of the equator. In both the temperate zones the regular winds must blow from west to east within these boundaries, as they incline towards the parallel of the atmospheric tide, and again restore the equilibrium of the atmosphere, which has been rarefied by the continual movement of the ærial tides. These consequences, deduced from theory, coincide perfectly with the course of the wind in the ocean. At land, and on confined seas, many other causes exist which contribute to change the regular direction of the winds, and to make them variable.

On the coasts, in the torrid zone, the wind blows for the most part from the sea towards the land. The reason of this is evident. The reflected rays of the sun, and other causes, heat and rarefy the air at land much more than at sea; and the direction of the wind always proceeds towards the rarefied part of the atmosphere. The places where the last mentioned winds separate themselves from the regular winds, must be absolutely calm. All this is confirmed by experience throughout the whole torrid zone, and particularly on the coast of Guinea.

Besides the determination of the course of the winds in consequence of the atmospheric flux and reflux, there are an immense number of local and accidental causes which have an influence on the creation and direction of the winds. One of the least irregular is the pressure and gravity of the atmosphere in the frigid zones, which must occasion a continued movement of the air towards the more rarefied parts, and at the same time a wind from the poles towards the equator. As soon, however, as this continual wind enters the temperate zones, its direction will be overcome and deranged by other local causes: an examination, however, of those winds not arising from the atmospheric tides, does not fall within the plan of this essay.

Those winds, on the other hand, which generally blow at sun-rise and sun-set, are a remarkable effect of the atmospheric

spheric tides produced by heat, as the gentle wind observed at the times of high water in the ocean, even during weather in other respects calm, proceeds from an atmospheric tide produced by attraction.

The difficulty of breathing which people experience in the torrid zone, under an atmospheric tide, arising from heat, must be ascribed as much to a rarefaction of the air thereby occasioned, as to the heat itself. A like effect, in regard to breathing, is perceived on high mountains; which however arises from the thinness of the air on these eminences, for here the heat can have no part in the rarefaction.

It happens sometimes, in warm countries, that during the atmospheric tides, the heat and rarefaction of the air increase to such a degree that they create scorching and suffocating winds, known under the name of *Sclanos*; the violence of which proceeds so far sometimes, that they destroy on the spot those who are immediately exposed to them. Abundant and horrid instances of this circumstance are quoted by the Abbé Richard in his Natural History of the Air and Meteors which have occurred in the Desarts of Arabia, in the neighbourhood of the Persian Gulph, and other places.

Besides these *solanos*, it is observed also, sometimes, that the wind suddenly rushes from all quarters to that point where the air is most rarefied, which occasions storms and hurricanes in that part by the mutual shock of the air and the vapours streaming in all directions towards one centre, and the winds then blow backwards again from this point to every quarter of the heavens till the equilibrium is restored. This effect, so natural and common, has long astonished those who never employed themselves in endeavouring to discover the cause.

In the last place, there is good reason for conjecturing that the same physical causes which produce the different atmospheric tides, with their consequences, contribute also to change the weather and temperature, and to produce at the same time a great many other meteorological phenomena.

II. *On the Nature and Construction of the Sun and Fixed Stars.* By WILLIAM HERSCHEL, LL.D. F.R.S.\*

AMONG the celestial bodies, the Sun is certainly the first which should attract our notice. It is a fountain of light that illuminates the world! It is the cause of that heat which maintains the productive power of Nature, and makes the earth a fit habitation for man! It is the central body of the planetary system; and what renders a knowledge of it still more interesting to us is, that the numberless stars which compose the universe, appear, by the strictest analogy, to be similar bodies. Their innate light is so intense, that it reaches the eye of the observer from the remotest regions of space, and forcibly claims his notice.

Now, if we are convinced that an enquiry into the nature and properties of the sun is highly worthy of our notice, we may also, with great satisfaction, reflect on the considerable progress that has already been made in our knowledge of this eminent body. It would require a long detail to enumerate all the various discoveries which have been made on this subject; I shall therefore content myself with giving only the most capital of them.

Sir Isaac Newton has shown that the sun, by its attractive power, retains the planets of our system in their orbits: he has also pointed out the method whereby the quantities of matter which it contains may be accurately determined. Dr. Bradley has assigned the velocity of the solar light with a degree of precision exceeding our utmost expectation. Galileo, Scheiner, Hevelius, Cassini and others have ascertained the rotation of the sun upon its axis, and determined the position of its equator. By means of the transit of Venus over the disk of the sun, our mathematicians have calculated its distance from the earth; its real diameter and magnitude; the density of the matter of which it is composed; and the fall of heavy bodies on its surface.

\* From the *Philosophical Transactions of the Royal Society* for 1794, Part I.

From the particulars here enumerated it is sufficiently obvious, that we have already a very clear idea of the vast importance and powerful influence of the sun on its planetary system: and if we add to this the beneficent effects we feel on this globe from the diffusion of the solar rays; and consider that, by well-traced analogies, the same effects have been proved to take place on other planets of this system, I should not wonder if we were induced to think that nothing remained to be added in order to complete our knowledge: and yet it will not be difficult to shew that we are still very ignorant, at least with regard to the internal construction of the sun. The various conjectures which have been formed on this subject, are evident marks of the uncertainty under which we have hitherto laboured.

The dark spots in the sun, for instance, have been supposed to be solid bodies revolving very near its surface. They have been conjectured to be the smoke of volcanoes, or the scum floating upon an ocean of fluid matter. They have also been taken for clouds. They were explained to be opaque masses swimming on the fluid matter of the sun, dipping down occasionally. It has been supposed that a fiery liquid surrounded the sun, and that, by its ebbing and flowing, the highest parts of it were occasionally uncovered, and appeared under the shape of dark spots; and that, by the return of the fiery liquid, they were again covered, and in that manner successively assumed different phases. The sun itself has been called a globe of fire, though perhaps metaphorically. The waste it would undergo by a gradual consumption, on the supposition of its being ignited, has been ingeniously calculated: and, in the same point of view, its immense power of heating the bodies of such comets as draw very near to it has been assigned.

The bright spots, or *faculæ*, have been called clouds of light, and luminous vapours. The light of the sun itself has been supposed to be directly invisible, and not to be perceived unless by reflection; though the proofs which are brought in support of that opinion seem to me to amount to no more than, what is sufficiently evident, that we cannot see when rays of light do not enter the eye.

But it is time to profit by the many valuable observations we are now in possession of. A list of successive eminent astronomers may be named, from Gallileo down to the present time, who have furnished us with materials for examination.

In supporting the ideas I shall propose in this paper, with regard to the physical construction of the sun, I have availed myself of the labours of all these astronomers, but have been induced thereto only by my own actual observation of the solar phenomena; which, besides verifying those particulars that had been already observed, gave me such views of the solar regions as led to the foundation of a very rational system. For, having the advantage of former observations, my latest reviews of the body of the sun were immediately directed to the most essential points; and the work was by this means facilitated, and contracted into a pretty narrow compass.

The following is a short extract of my observations on the sun, to which I have joined the consequences I now believe myself entitled to draw from them. When all the reasonings on the several phenomena are put together, and a few additional arguments taken from analogy, which I shall also add, are properly considered, it will be found that a general conclusion may be made which seems to throw a considerable light on our present subject.

In the year 1779 there was a spot on the sun which was large enough to be seen with the naked eye. By a view of it with a seven feet reflector, charged with a very high power, it appeared to be divided into two parts. The largest of the two, on the 19th of April, measured  $1'8''\cdot06$  in diameter; which is equal in length to more than 31 thousand miles. Both together must certainly have extended above 50 thousand.

The idea of its being occasioned by a volcanic explosion violently driving away a fiery fluid, which on its return would gradually fill up the vacancy, and thus restore the sun, in that place, to its former splendour, ought to be rejected on many accounts. To mention only one, the great extent of the spot is very unfavourable to such a supposition. Indeed a much  
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less violent and less pernicious cause may be assigned to account for all the appearances of the spot. When we see a dark belt near the equator of the planet Jupiter, we do not recur to earthquakes and volcanoes for its origin. An atmosphere, with its natural changes, will explain such belts. Our spot on the sun may be accounted for on the same principles. The earth is surrounded by an atmosphere composed of various elastic fluids. The sun also has its atmosphere; and if some of the fluids which enter into its composition should be of a shining brilliancy, in the manner that will be explained hereafter, while others are merely transparent, any temporary cause which may remove the lucid fluid will permit us to see the body of the sun through the transparent ones. If an observer were placed on the moon, he would see the solid body of the earth only in those places where the transparent fluids of our atmosphere would permit him. In others, the opaque vapours would reflect the light of the sun without permitting his view to penetrate to the surface of our globe. He would probably also find that our planet had occasionally some shining fluids in its atmosphere; as, not unlikely, some of our northern lights might not escape his notice, if they happened in the unenlightened part of the earth, and were seen by him in his long dark night. Nay, we have pretty good reason to believe, that probably all the planets emit light in some degree; for the illumination which remains on the moon in a total eclipse cannot be entirely ascribed to the light which may reach it by the refraction of the earth's atmosphere. For instance, in the eclipse of the moon October 22, 1790, the rays of the sun refracted by the atmosphere of the earth towards the moon, admitting the mean horizontal refraction to be  $30' 50'' \cdot 8$ , would meet in a focus 189 thousand miles beyond the moon; so that consequently there could be no illumination from rays refracted by our atmosphere. It is, however, not improbable, that about the polar regions of the earth there may be refraction enough to bring some of the solar rays to a shorter focus. The distance of the moon at the time of the eclipse would require a refraction of  $54' 6''$ , equal to its horizontal parallax

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at that time, to bring them to a focus so as to throw light on the moon.

The unenlightened part of the planet Venus has also been seen by different persons, and not having a satellite, those regions that are turned from the sun cannot possibly shine by a borrowed light; so that this faint illumination must denote some phosphoric quality of the atmosphere of Venus.

In the instance of our large spot on the sun, I conclude from appearances that I viewed the real body of the sun itself, of which we rarely see more than its shining atmosphere.

In the year 1783 I observed a fine large spot, and followed it up to the edge of the sun's limb. Here I took notice that the spot was plainly depressed below the surface of the sun; and that it had very broad shelving sides. I also suspected some part, at least, of the shelving sides to be elevated above the surface of the sun; and observed that, contrary to what usually happens, the margin of that side of the spot which was farthest from the limb was the broadest.

The luminous shelving side of a spot may be explained by a gentle and gradual removal of the shining fluid, which permits us to see the globe of the sun. As to the uncommon appearance of the broadest margin being on that side of the spot which was farthest from the limb when the spot came near the edge of it, we may surmise that the sun has inequalities on its surface, which may possibly be the cause of it. For, when mountainous countries are exposed, if it should chance that the highest part of the landscape are situated so as to be near that side of the margin, or penumbra of the spot, which is towards the limb, it may partly intercept our view of it when the spot is seen very obliquely. This would require elevations at least five or six hundred miles high; but considering the great attraction exerted by the sun upon bodies at its surface, and the slow revolution it has upon its axis, we may readily admit inequalities to that amount. From the centrifugal force at the sun's equator, and the weight of bodies at its surface, I compute that the power of throwing down a mountain by the exertion of the former, balanced by the  
superior



superior force of keeping it in its place of the latter, is near  $6\frac{1}{2}$  times less on the sun than on our equatorial regions; and as an elevation similar to one of three miles on the earth would not be less than 334 miles on the sun, there can be no doubt but that a mountain much higher would stand very firmly. The little density of the solar body seems also to be in favour of the height of its mountains; for, *cæteris paribus*, dense bodies will sooner come to their level than rare ones. The difference in the vanishing of the shelving side, instead of explaining it by mountains, may also, and perhaps more satisfactorily, be accounted for from the real difference of the extent, the arrangement, the height, and the intensity of the shining fluid, added to the occasional changes that may happen in these particulars during the time in which the spot approaches to the edge of the disk. However, by admitting large mountains on the face of the sun, we shall account for the different opinions of two eminent astronomers; one of whom believed the spots depressed below the sun, while the other believed them elevated above it. For it is not impossible that some of the solar mountains may be high enough occasionally to project above the shining elastic fluid, when, by some agitation, or other cause, it is not of the usual height: and this opinion is much strengthened by the return of some remarkable spots which served Cassini to ascertain the period of the sun's rotation. A very high country, or chain of mountains, may oftener become visible, by the removal of the obstructing fluid, than the lower regions, on account of its not being so deeply covered with it.

In the year 1791 I examined a large spot on the sun, and found it evidently depressed below the level of the surface; about the dark part was a broad margin, or plane, of considerable extent, less bright than the sun, and also lower than its surface. This plane seemed to rise, with shelving sides, up to the place where it joined the level of the surface.

In confirmation of these appearances, I carefully remarked that the disk of the sun was visibly convex: and the reason of my attention to this particular, was my being already long acquainted with a certain optical deception, that takes place now and then when we view the moon; which is, that all the

the elevated spots on its surface will seem to be cavities, and all cavities will assume the shape of mountains. But then, at the same time, the moon, instead of having the convex appearance of a globe, will seem to be a large concave portion of an hollow sphere. As soon as, by the force of imagination, you drive away the fallacious appearance of a concave moon, you restore the mountains to their protuberance, and sink the cavities again below the level of the surface. Now, when I saw the spot lower than the shining matter of the sun, and an extended plane, also depressed, with shelving sides rising up to the level, I also found that the sun was convex, and appeared in its natural globular state. Hence I conclude that there could be no deception in those appearances.

How very ill would this observation agree with the ideas of solid bodies bobbing up and down in a fiery liquid? with the smoke of volcanoes, or scum upon an ocean? And how easily it is explained upon our foregoing theory. The removal of the shining atmosphere, which permits us to see the sun, must naturally be attended with a gradual diminution on its borders: an instance of a similar kind we have daily before us, when through the opening of a cloud we see the sky, which generally is attended by a surrounding haziness of some short extent; and seldom transits, from a perfect clearness, at once to the greatest obscurity.

*Aug. 26, 1792.* I examined the sun with several powers, from 90 to 500. It appears evidently that the black spots are the opaque ground, or body of the sun; and that the luminous part is an atmosphere, which, being interrupted or broken, gives us a transient glimpse of the sun itself. My 7-feet reflector, which is in high perfection, represents the spots, as it always used to do, much depressed below the surface of the luminous part.

*Sept. 2, 1792.* I saw two spots in the sun with the naked eye. In the telescope I found they were clusters of spots, with many scattered ones besides. Every one of them was certainly below the surface of the luminous disk.

*Sept. 8, 1792.* Having made a small speculum, merely brought to a perfect figure upon hones without polish, I found that, by stifling a great part of the solar rays, my ob-

ject-Speculum would bear a greater aperture; and thus enabled me to see with more comfort, and less danger. The surface of the sun was unequal; many parts of it being elevated, and others depressed. This is here to be understood of the shining surface only, as the real body of the sun can probably be seldom seen otherwise than in its black spots.

It may not be impossible, as light is a transparent fluid, that the sun's real surface also may now and then be perceived; as we see the shape of the wick of a candle through its flame, or the contents of a furnace in the midst of the brightest glare of it: but this, I should suppose, will only happen where the lucid matter of the sun is not very accumulated.

Sept. 9, 1792. I found one of the dark spots in the sun drawn pretty near the preceding edge. In its neighbourhood I saw a great number of elevated bright places, making various figures: I shall call them *faculae*, with Hevelius; but without assigning to this term any other meaning than what it will hereafter appear ought to be given to it. I see these *faculae* extended, on the preceding side, over about 1-6th part of the sun; but so far from resembling torches, they appear to me like the shrivelled elevations upon a dried apple, extended in length, and most of them are joined together, making waves, or waving lines.

By some good views in the afternoon, I find that the rest of the surface of the sun does not contain any *faculae*, except a few on the following, and equatorial part of the sun. Towards the north and south I see no *faculae*: there is all over the sun a great unevenness in the surface, which has the appearance of a mixture of small points of an unequal light; but they are evidently an unevenness or roughness of high and low parts.

Sept. 11, 1792. The *faculae*, in the preceding part of the sun, are much gone out of the disk, and those in the following are come on. A dark spot also is come on with them.

Sept. 13, 1792. There are a great number of *faculae* on the equatorial part of the sun, towards the preceding and following parts. I cannot see any towards the poles; but a roughness is visible every where.

*Sept. 16, 1792.* The sun contains many large faculæ on the following side of its equator, and also several on the preceding side. I perceive none about the poles. They seem generally to accompany the spots; and probably, as the faculæ certainly are elevations, a great number of them may occasion neighbouring depressions, that is to say, dark spots.

The faculæ being elevations, very satisfactorily explains the reason why they disappear towards the middle of the sun, and re-appear on the other margin; for, about the place where we lose them, they begin to be edge-ways to our view; and if between the faculæ should lie dark spots, they will most frequently break out in the middle of the sun, because they are no longer covered by the side-views of these faculæ.

*Sept. 22, 1792.* There are not many faculæ in the sun, and but few spots; the whole disk, however, is very much marked with roughness, like an orange. Some of the lowest parts of the inequalities are blackish.

*Sept. 23, 1792.* The following side of the sun contains many faculæ near the limb. They take up an arch of about 50 degrees. There are likewise some on the preceding side. The north and south is rough as usual, but differently disposed. The faculæ are ridges of elevations above the rough surface.

*Feb. 23, 1794.* By an experiment I have just now tried, I find it confirmed that the sun cannot be so distinctly viewed with a small aperture and faint darkening glasses, as with a large aperture and stronger ones; this latter is the method I always use.

One of the black spots on the preceding margin, which was greatly below the surface of the sun, had, next to it, a protuberant lump of shining matter, a little brighter than the rest of the sun.

About all the spots, the shining matter seems to have been disturbed; and is uneven, lumpy, and zig-zagged in an irregular manner.

I call the spots black, not that they are entirely so, but merely to distinguish them; for there is not one of them to-day, which is not partly, or entirely, covered over with whitish and unequally bright nebulosity, or cloudiness. This, in

in many of them, comes near to an extinction of the spot; and in others, seems to bring on a subdivision.

*Sept. 28, 1794.* There is a dark spot in the sun on the following side. It is certainly depressed below the shining atmosphere, and has shelving sides of shining matter, which rise up higher than the general surface, and are brightest at the top. The preceding shelving side is rendered almost invisible by the overhanging of the preceding elevations, while the following is very well exposed; the spot being apparently such in figure as denotes a circular form viewed in an oblique direction.

Near the following margin are many bright elevations, close to visible depressions. The depressed parts are less bright than the common surface.

The penumbra, as it is called, about this spot, is a considerable plane, of less brightness than the common surface, and seems to be as much depressed below that surface as the spot is below the plane.

Hence, if the brightness of the sun is occasioned by the lucid atmosphere, the intensity of the brightness must be less where it is depressed; for light, being transparent, must be the more intense the more it is deep.

*Oct. 12, 1794.* The whole surface of the sun is diversified by inequality in the elevation of the shining atmosphere. The lowest parts are every where darkest; and every little pit has the appearance of a more or less dark spot.

A dark spot, which is on the preceding side, is surrounded by very great inequalities in the elevation of the lucid atmosphere; and its depression below the same is bounded by an immediate rising of very bright light.

*Oct. 13, 1794.* The spot in the sun I observed yesterday is drawn so near the margin, that the elevated side of the following part of it hides all the black ground, and still leaves the cavity visible, so that the depression of the black spots, and the elevation of the faculæ, are equally evident.

[To be continued.]

III. *On the Production of Cast Iron, and the Operations of the Blast-Furnace.* By Mr. DAVID MUSHET, of the Clyde Iron-Works. Communicated by the Author.

IT was my wish, in the papers preceeding this communication, to convey a clear and competent idea of the nature of iron as a metal; as also of ores, and iron-stones in general. I have endeavoured to explain, upon principles grounded on experiment, the chief agents of change in the smelting operation, so far as they affect the quality of the materials prepared. I have aimed at perspicuity rather than minuteness, which often becomes tedious; and although the facts may not be conveyed in a style so popular as communications, which profess a conveyance of practical truths, generally are, yet, I trust, this will form no real bar to their utility. When facts are to be learned, and principles satisfactorily explained, it is surely best to begin by the examination of simple causes; and tracing their agency according to their quantities, relative proportions, and affinities. These facts will undoubtedly, in the end, be more radically understood by using such phrases, or signs, as denote in each a respective quality affixed in consequence of a knowledge of its properties and effects. In short, if the language and reasoning of our manufactories is ever to become scientific; if philosophy and chemistry are ever to become of general utility in the perfection of any branch; then those truths which constitute the foundation of all science are not to be rejected, though clothed in a dress which at first sight appears discordant to our habits, or burdensome to our memory.

The operations I am about to describe have never as yet received any explanation consonant to true philosophy or chemical facts; yet there are few which present a more beautiful chain of affinities, decomposition, and recombination, than the manufacture of iron in all its various stages. An extensive foundry is a laboratory fraught with phenomena of the most interesting nature in chemistry and natural philosophy: are we not then justly surprised to find that prejudice

still

still reigns there; and that the curious manipulations of these regions are still shrouded with error and misconception; as if their dingy structure forbade the entrance of genius, or consigned her laborious unlettered sons to an endless stretch of mental obscurity?

The plate of the blast-furnace, given in the preceding Number, having a full description appropriated to it, I shall proceed to detail the train of preparation necessary before the furnace is brought to produce good melting iron.

The furnace being finished, the bottom and sides of it, for two feet up the square funnel, receive a lining of common bricks upon edge, to prevent the stone from shivering or mouldering when the fire comes in contact with it. On the front of the furnace is erected a temporary fire-place, about four feet long, into the bottom of which are laid corresponding bars. The side-walls are made so high as to reach the under-surface of the tympan-stone; excepting a small space, which afterwards receives an iron plate of  $1\frac{1}{2}$  inches thickness, by way of a cover: this also preserves the tympan-stone from any injury it might sustain by being in contact with the flame. A fire is now kindled upon the bars, and is fed occasionally with small coals. As the whole cavity of the furnace serves as a chimney for this fire, the draught in consequence is violent, and the body of heat carried up is very considerable. In the course of three weeks the furnace will thus become entirely free from damp, and fit for the reception of the materials: when this is judged proper the fire-place is removed, but the interior bricks are allowed to remain till the operation of blowing commences. Some loose fuel is then thrown upon the bottom of the furnace, and a few baskets of cokes are introduced; these are allowed to become thoroughly ignited before more are added. In this manner the furnace is gradually filled; sometimes entirely full, and at other times 5-8ths or 3-4ths full. The number of baskets full depend entirely upon the size of the furnace: that in the plate will contain 900 baskets. If the coal is splint, the weight of each basket-full will be nearly 11 lb.  $\times$  900 = 99,000 lb. cokes. As this quality of cokes is made with a loss of nearly 50 per cent. the original weight in raw

coals

coals will be equal to 198,000 lb. When we reflect that this vast body of ignited matter is replaced every third day, when the furnace is properly at work, a notion may be formed of the immense quantity of materials requisite, as also the consequent industry exerted to supply one or more furnaces for the space of one year.

When the furnace is sufficiently heated throughout, specific quantities of cokes, iron-stone, and blast-furnace cinders are added: these are called charges. The cokes are commonly filled in baskets, which, at all the various iron-works, are nearly of a size. The weight of a basket, however, depends entirely upon the nature and quality of the coal, being from 70 to 112 lb. each \*. The iron-stone is filled into boxes, which, when moderately heaped, contain 56 lb. of torrefied iron-stone; they often exceed this when the stone has been severely roasted. The first charges which a furnace receives, contain but a small proportion of iron-stone to the weight of cokes: this is afterwards increased to a full burden, which is commonly 4 baskets cokes, 320 lb.; 2 boxes iron-stone, 112 lb.; 1 box blast-furnace cinders, 60 or 70 lb.†. At new works, where these cinders cannot be obtained, a similar quantity of limestone is used.

The descent of the charge, or burden, is facilitated by opening the furnace below two or three times a-day, throwing out the cold cinders, and admitting, for an hour at a time, a body of fresh air. This operation is repeated till the approach of the iron-stone and cinder, which is always announced by a partial fusion, and the dropping of lava through the iron bars, introduced to support the incumbent materials while those on the bottom are carried away. The filling above is regularly continued, and when the furnace at the

\* This same variety in the coal renders it almost impossible, under one description, to give a just idea of the proportions used at various blast-furnaces: to avoid being too diffuse, I shall confine my description connected with a coal of a medium quality, or a mixture of splint and free-coal, a basket of which will weigh from 78 lb. to 84 lb.

† A preference at first is always given to blast-furnace cinders in place of lime; being already vitrified, they are of much easier fusion, and tend to preserve the surface of the hearth by glazing it over with a black vitrid crust.



top has acquired a considerable degree of heat, it is then judged time to introduce the blast; the preparations necessary for which are the following:—

The dam-stone is laid in its place firmly imbedded in fire-clay; the dam-plate is again imbedded on this with the same cement, and is subject to the same inclination. On the top of this plate is a slight depression, of a curved form, towards that side farthest distant from the blast, for the purpose of concentrating the scoria, and allowing it to flow off in a connected stream, as it tends to surmount the level of the dam. From this notch to the level of the floor a declivity of brick-work is erected, down which the scoria of the furnace flows in large quantities. The opening betwixt the dam and side-walls of the furnace, called the *fauld*, is then built up with sand, the loose bricks are removed, and the furnace bottom is covered with powdered lime or charcoal-dust. The ignited cokes are now allowed to fall down, and are brought forward with iron bars nearly to a level with the dam. The space between the surface of the cokes and the bottom of the tymp-plate is next rammed hard with strong binding sand; and these cokes, which are exposed on the outside, are covered with coke-dust. These precautions being taken, the tuyere-hole is then opened and lined with a soft mixture of fire-clay and loam: the blast is commonly introduced into the furnace at first with a small discharging-pipe, which is afterwards increased as occasion may require. In two hours after blowing, a considerable quantity of lava will be accumulated; iron bars are then introduced, and perforations made in the compressed matter at the bottom of the furnace; the lava is admitted to all parts of the hearth, and soon thoroughly heats and glazes the surfaces of the fire-stone. Shortly after this it rises to a level with the notch in the dam-plate, and by its own accumulation, together with the forcible action of the blast, it flows over. Its colour is at first black; its fracture dense, and very ponderous; the form it assumes in running off is flat and branched, sometimes in long streams, and at other times less extensive. If the preparation has been well conducted, the colour of the cinder will soon change to white; and the metal, which in the state of an oxyde formerly

coloured

coloured it, will be left in a disengaged state in the furnace. When the metal has risen nearly to a level with the dam, it is then let out by cutting away the hardened loam of the fauld, and conveyed by a channel, made in sand, to its proper destination; the principal channel, or runner, is called the *sow*, the lateral moulds are called the *pigs*.

In six days after the commencement of blowing, the furnace ought to have *wrought herself clear*, and have acquired capacity sufficient to contain from 5000 to 7000 weight of iron. The quality ought also to be richly carbonated, so as to be of value and estimation in the pig-market. At this period, with a quality of coal as formerly mentioned, the charge will have increased to the following proportions:—5 baskets cokes, 400lb.; 6 boxes iron-stone, 336lb.; 1 box limestone, 100lb.

An analysis of the smelting operation, and the tendency which the individual agents have to produce change in the quality and quantity of the iron, come next under consideration. Let us, however, first notice the characteristic features exhibited by the different kinds of iron while in fusion, whereby the quality of the metal may be justly defined.

When fine (No. 1.) or supercarbonated crude iron is run from the furnace, the stream of metal, as it issues from the fauld, throws off an infinite number of brilliant sparkles of carbon. The surface is covered with a fluid pellicle of carburet of iron, which, as it flows, rears itself up in the most delicate folds: at first the fluid metal appears like a dense, ponderous stream, but, as the collateral moulds become filled, it exhibits a general rapid motion from the surface of the pigs to the centre of many points; millions of the finest undulations move upon each mould, displaying the greatest nicety and rapidity of movement, conjoined with an uncommonly beautiful variegation of colour, which language is inadequate justly to describe. Such metal, in quantity, will remain fluid for twenty minutes after it is run from the furnace, and when cold will have its surface covered with the beautiful carburet of iron, already mentioned, of an uncommonly rich and brilliant appearance. When the surface of the metal is not carbureted, it is smooth like forged iron, and  
always

always convex. In this state iron is too rich for melting without the addition of coarse metal, and is unfit to be used in a cupola furnace for making fine castings, where thinness and a good skin are requisite.

No. 4, or oxygenated crude iron, when issuing from the blast-furnace, throws off from all parts of the fluid surface a vast number of metallic sparks: they arise from a different cause than that exerted in the former instance. The extreme privation of carbon renders the metal subject to the combination of oxygen so soon as it comes into contact with atmospheric air. This truth is evidently manifested by the ejection of small spherules of iron from all parts of the surface: the deflagration does not, however, take place till the globule has been thrown two or three feet up in the air; it then inflames and separates, with a slight hissing explosion, into a great many minute particles of brilliant fire. When these are collected they prove to be a true oxyde of iron, but so much saturated with oxygen as to possess no magnetic obedience. The surface of oxygenated iron, when running, is covered with waving flakes of an obscure smoky flame, accompanied with a hissing noise; forming a wonderful contrast with the fine rich covering of plumbago in the other state of the metal, occasionally parting and exhibiting the iron in a state of the greatest apparent purity, agitated in numberless minute fibres, from the abundance of the carbon united with the metal.

When iron thus highly oxygenated comes to rest, small specks of oxyde begin to appear floating upon the surface: these increase in size; and when the metal has become solid, the upper surface is found entirely covered with a scale of blue oxyde of various thicknesses, dependent upon the stage of oxygenation or extreme privation of carbon. This oxyde, in common, contains about 15 *per cent.* of oxygen, and is very obedient to the magnet. In place of a dark blue smooth surface, convex and richly carbonated, the metal will exhibit a deep, rough, concave face, which, when the oxyde is removed, presents a great number of deep pits. This iron in fusion stands less convex than carbonated iron, merely because it is less susceptible of a state of extreme division; and

indeed it seems a principle in all metallic fluids, that they are convex in proportion to the quantity of carbon with which they are saturated. This iron flows dead and ponderous, and rarely parts in shades but at the distance of some inches from each other.

This is a slight sketch of the appearance of the two extreme qualities of crude, or pig-iron, when in a state of fusion. According to the division formerly made, there still remains two intermediate stages of quality to be described: these are, carbonated and carbo-oxygenated iron; that is, No. 2 and 3 of the manufacturers. Carbonated iron exhibits, like No. 1, a beautiful appearance in the runner and pig. The breakings of the fluid, in general, are less fine; the agitation less delicate; though the division of the fluid is equal, if not beyond that of the other. When the internal ebullition of the metal is greatest, the undulating shades are smallest and most numerous: sometimes they assume the shape of small segments; sometimes fibrated groups; and at other times minute circles, of a mellow colour than the ground of the fluid. The surface of this metal, exposed to external air, when cooling is generally slightly convex, and full of punctures: these, in iron of a weak and fusible nature, are commonly small in the diameter, and of no great depth. In strong metal, the punctures are much wider and deeper. This criterion, however, is not infallible, when pig-iron of different works is taken collectively. At each individual work, however, that iron will be strongest whose honey-combs are largest and deepest.

Carbo-oxygenated, or No. 3, pig-iron, runs smoothly, without any great degree of ebullition or disengagement of metallic sparks. The partings upon its surface are longer, and at greater distances from each other than in the former varieties; the shape they assume is either elliptical, circular, or curved. In cooling, this metal acquires a considerable portion of oxyde; the surface is neither markedly convex nor concave; the punctures are less, and frequently vanish altogether. Their absence, however, is no token of a smooth face succeeding: in qualities of crude iron oxygenated beyond this, I have already mentioned that a concave surface

is the consequence of the extreme absence of carbon; and that, in proportion as this principle is absent, the surface of the iron acquires roughness and asperity.

It may perhaps be proper here to mention, once for all, that although, for convenience, the manufacturer has, from a just estimation of the value of the metal in a subsequent manufacture, affixed certain numbers for determinate qualities of iron, yet it is difficult to say at what degree of saturation of carbon each respective term commences: suffice it then to say, that the two alterative principles, oxygen and carbon, form two distinct classes, that in which oxygen predominates, and that in which carbon predominates; the latter comprehends No. 1 and 2 of the manufacturers, the former includes oxygenated, white and mottled; and the equalisation of these mixtures form, as has already been noticed, the variety of carbo-oxygenated crude iron.

I shall now observe some things relative to the various faces which crude iron assumes. No. 1 and 2, with their intermediate qualities, possess surfaces more or less convex, and frequently with thin blisters: this we attribute to the presence of carbon, which being plentifully interspersed betwixt and throughout the particles of the metal, the tendency which the iron has to shrink in cooling is entirely done away; it tends to distend the aggregate of the mass, and to give a round face, by gradually elevating the central parts of the surface, which are always last to lose their fluidity.

Again, that quality of iron known by the name of No. 3, or carbo-oxygenated, is most commonly found with a flat surface. If we still farther trace the appearance of the surface of pig-iron, when run from the furnace, we shall find No. 4, either with a white or mottled fracture, possessed of concave faces rough and deeply pitted. Beyond this it may be imagined that every degree of further oxygenation would be productive of a surface deeper in the curve, and rougher, with additional asperities. The contrary is the case: when crude iron is so far debased as to be run from the furnace in clotted lumps highly oxygenated, the surface of the pigs is found to be more convex than that of No. 1 iron; but then

the fracture of such metal presents an impure mass covered on both faces with a mixture of oxydated iron, of a blueish colour, nearly metallic. In short, this quality of iron is incapable of receiving such a degree of fluidity as to enable us to judge whether the convexity of its surface is peculiar to its state, or is owing to its want of division as a fluid, whereby the gradual consolidation of the metal is prevented.

These features sufficiently distinguish betwixt the various qualities of crude iron after they are obtained from the blast-furnace: there are, however, criterions not less infallible, whereby we can prejudge the quality of the metal many hours before it is run from the furnace. These are the colour and form of the scoria, the colour of the vitrid crust upon the working bars, and the quantity of carburet which is attached to it. The variety of colour and form in the cinder almost universally indicate the quality of the metal on the hearth. Hence, from a long course of experience, have arisen the following denominations: "Cinder of sulphury iron;" "Cinder of No. 1, No. 2, and No. 3;" and "Cinder of ballast iron." Although at different works, from local circumstances, the same kind of scoria may not indicate precisely the same quality of iron, yet the difference is so small that the following description of the various cinders may convey a very just idea of their general appearance.

When the scoria is of a whitish colour and short form, branching from the notch of the dam, and emitting from its stream beautiful sparks of ignited carbon, resembling those ejected from a crucible of cast steel in fusion, exposed to external air, or to the combustion of fine steel filings in a white flame; if, when issuing from the orifice of the furnace, it is of the purest white colour, possessing no tenacity, but in a state of the greatest fluid division, and, when cold, resembles a mass of heavy torrefied spar, void of the smallest vitrid appearance, hard and durable, it is then certain that the furnace contains *fulbury iron*, i. e. super-carbonated iron. At blast-furnaces, where a great quantity of air is thrown in *per minute*, super-carbonated crude iron will be obtained with a cinder of a longer form, with a rough flinty fracture towards the outside of the column.

That

That cinder which indicates the presence of carbonated iron in the hearth of the furnace, forms itself into circular compact streams, which become consolidated and inserted into each other; these are in length from three to nine feet. Their colour, when the iron approaches the first quality, is a beautiful variegation of white and blue enamel, forming a wild profusion of the elements of every known figure; the blues are lighter or darker according to the quantity of the metal and the action of the external air while cooling. When the quality of the pig-iron is sparingly carbonated, the blue colour is less vivid, less delicate; and the external surface rougher, and more sullied with a mixture of colour. The same scoria, when fused in vessels which are allowed to cool gradually, parts with all its variety of light and shade, and becomes of a yellowish colour, sometimes nearly white when the quantity of incorporated metal has been small.

The cinder which is emitted from the blast-furnace when carbo-oxygenated (or No. 3,) iron is produced, assumes a long zig-zag form. The stream is slightly convex in the middle; broad, flat, and obliquely furrowed towards the edges. The end of the stream frequently rears itself into narrow tapered cones, to the height of six or eight inches: these are generally hollow in the centre, and are easily demolished, owing to their excessive brittleness. The colour of this lava is very various; for the most part it is pale yellow mixed with green. Its tenacity is so great, that if, while fluid, a small iron hook is inserted into it at a certain degree of heat, and then drawn from it with a quick but steady motion, 20 to 30 yards of fine glass thread may be formed with ease. If the colours are vivid and variegated, the thread will possess, upon a minute scale, all the various tints of colouring which is found in the columnar mass. When by accident a quantity of this lava runs back upon the discharging-pipe, it is upon the return of the blast impelled with such velocity as to be blown into minute delicate fibres, smaller than the most ductile wire; at first they float upon the air like wool, and when at rest very much resemble that substance.

The presence of oxygenated crude iron (No. 4,) on the furnace-

furnace-hearth, is indicated by the lava resolving itself into long streams, sometimes branched, sometimes columnar, extending from the notch to the lowest part of the declivity; here it commonly forms large, flat, hollow cakes, or inclines to form conical figures: these are, however, seldom perfect; for the quantity of fluid lava, conveyed through the centre of the column, accumulates faster than the external sides of the cone are consolidated; and thus, when the structure is only half finished, the small crater vomits forth its superabundant lava, and is demolished. The current of such lava falls heavily from the dam as if surcharged with metal, and emits dark red sparks resembling the agitation of straw embers. Its colour is still more varied than the former descriptions of scoriæ, and is found changing its hues through a great variety of greens shaded with browns. Another variety of scoria, which indicates the same quality of iron, assumes a similar form; but has a black ground colour mixed with browns, or is entirely black. When the latter colour prevails, the texture of the cinder becomes porous; the quantity of iron left is now very considerable, and such as will be easily extracted in the assay-furnace with proper fluxes. In cases of total derangement in the furnace, the scoria will still retain this black colour, although the quantity of metal may amount to 25 *per cent.*; the fracture, however, becomes dense, and its specific gravity increases in proportion to the quantity of metal it holds incorporated.

The next source of information, as to the quality of the iron in the furnace, is to be got from the colour of the scoria upon the working bars, which are from time to time inserted to keep the furnace free from lumps, and to bring forward the scoria. When super-carbonated crude iron is in the hearth, the vitrid crust upon the bars will be of a black colour and smooth surface, fully covered with large and brilliant plates of plumbago.

As the quality of the metal approaches to No. 2 (carbonated), the carburet upon the scoria decreases both in point of quantity and size.

When carbo-oxygenated iron (No. 3) is in the furnace, the working bars are always coated with a lighter co-



oured scoria than when the former varieties exist; a speck of plumbago is now only found here and there, and that of the smallest size. When the quality of the metal is oxygenated (No. 4.), not only have the plates of carburet disappeared, but also the coally colour on the external surface of the scoria; what now attaches to the bars, is nearly of the same nature and colour as the lava emitted at the notch of the dam.

These criterions are infallible; for, as the fusibility or carbonation of the metal is promoted in a direct ratio to the comparative quantity of the coally principle present in the furnace, so in the same proportion will the vitrid crust encircling the working bars exhibit the presence of that principle in the furnace.

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IV. *Agenda, or a Collection of Observations and Researches, the Results of which may serve as the Foundation for a Theory of the Earth.* By M. DE SAUSSURE.

[Continued from Page 29.]

CHAP. XXII.

*Errors to be avoided in Observations respecting Geology.*

1. **T**HERE are some errors into which people may readily fall when they have not had long experience in any given kind of observation, and against which it is of importance to put beginners at least on their guard.

2. One may be readily deceived in regard to the relative distances of remote objects. All the stars and planets appear to be at an equal distance from us. Distant mountains all appear to be in the same plain. Thus those which are situated very far behind the rest, seem to form one body with them; so that people believe they see continued and uninterrupted chains when there are really none, and where the mountains, on the contrary, are insulated.

The absolute distance of objects, even when not very remote, is equally difficult to be ascertained on high mountains,

tains, where the transparency of the air, and the absence of vapours, destroy the aerial perspective. I have often imagined that I had only two or three hundred steps to make in order to reach a summit, the distance of which from me was more than a league in a straight line.

3. There are a great many errors in regard to strata. Their great thickness may make one believe that there are none where they really exist. In the like manner, if the vertical strata, or those only very much inclined, present their planes to the eye of the observer, he will think he sees shapeless and indivisible masses; while, if their sections were seen, their divisions would readily be distinguished. A mountain then must be seen under aspects that intersect each other at right angles before we can pronounce that it is not divided by strata.

4. At other times accidental fissures, but produced however by a cause which is common to them, exhibit the appearance of strata when there are none; or when, if there are, their situation is very different from that of those strata. It is the internal tissue of the stone only which in many cases can determine whether the divisions observed are the interfices between strata or mere fissures; because the strata are constantly parallel to the internal laminæ, or schistous texture of the stone. Crystals, the lamellated texture of which may sometimes be confounded with a schistous texture, may afford an exception to this rule, by presenting laminæ perpendicular to the planes of the strata; but it is not difficult to distinguish them.

5. One may also form an erroneous opinion respecting the direction of a mountain, or of its strata, when the eye is not situated in their prolongation, or at least near it.

6. The apparent situation of the strata may also lead into an error. They appear horizontal even when they are very much inclined, and when they are not seen but in a section formed by a plane parallel to the common section of their planes with the horizon. It is impossible to judge of their inclination, and to measure it with certainty, but on a section perpendicular to the common section, which I have just mentioned.

6. A. The

6. A. The greatest error, however, is that which may be committed in regard to the super-position of strata. I have often seen novices in the study of mountains believe that one stratum reposed on another; one of granite, for example, on one of slate; because they found slate at the bottom of the mountain, and granite at the top; while the slate was only laid against the base of the mountain, and the granite, on the other hand, was sunk in the earth far below the slate. We must not then say, that a stratum is situated below another, but when we really see it extending itself below it.

7. And even when we distinctly see a rock placed above another, we must examine whether that which is uppermost does not occupy that situation accidentally; whether it has not slipped, or rolled down, from a more elevated mountain; and, in the last place, though they may be closely connected, one must examine whether their present situation is really the same in which they were formed, and whether they have not been reversed, and united accidentally in a situation contrary to that of their original formation.

8. One is frequently deceived, also, in regard to the nature of stones and of mountains. Though a well-accustomed eye may often judge at some, and even a considerable distance, of the kind of stone of which a mountain is composed, such judgment is however often erroneous: mountains of granite, or *gneiss*, tender and destructible, often assume, at a distance, the round form of secondary mountains; sometimes, also, mountains of calcareous stone, hard of their kind, and in strata either vertical or very much inclined, present the bold forms, the peaks, and sharp-angled indentations of the granite summits.

9. People are often deceived even on a near view. A stone may have a foreign covering of mica; for example, while the interior part is of a very different nature.

10. Effervescence with the nitrous acid is commonly considered as a certain character of calcareous stone; but this character may be deceptive, since barytes and magnesia effervesce also\*: and we must not consider it enough to touch

\* And, on the other hand, there are calcareous stones which do not effervesce.

a stone with the nitrous acid, or to let fall a drop of the acid on its surface, since the absorbing earth, whatever it is, may be only disseminated between argillaceous or siliceous particles. We must therefore immerse a fragment of the stone in a quantity of the acid sufficient to dissolve it entirely, if it be wholly soluble, and observe whether there remains any residuum that withstands solution.

11. The action of the air and of meteors often gives fossil appearances absolutely different from those which they had before they were subjected to it. We must not then be satisfied with a superficial examination: we must sound the rocks to the quick where the action of meteoric agents has not penetrated.

12. People are often deceived, also, in considering compound stones as simple stones, when the composition of them does not manifest itself on the first view, either on account of the smallness of their composing parts, or because some of these parts are each inclosed separately in a covering which conceals the interior of them. One may guard against this error by observing the fossil in the sun with strong magnifying glasses, after having moistened its surface with water or the nitrous acid, and still better by exposing it gradually to the flame of the blow-pipe.

13. People are often deceived in regard to crystallisation, either in the true form of the crystals, or, above all, in taking for real crystals parasite crystals, or such as have been formed in the moulds made by crystals of another kind. Thus we see crystals of quartz, petro-felix, and jasper, formed in the moulds made by calcareous crystals, and which have assumed the form of the latter.

14. In regard to errors occasioned by ignorance of the distinctive characters of fossils, and of the names proper for them, the only means of avoiding such errors is to study with care good authors; and, above all, collections formed, or at least arranged and titled, by able mineralogists.

15. But when the slightest doubt is entertained in regard to the denomination which ought to be given to any fossil, an exact description must be made either of its external characters or its most striking physical properties, such as weight

and

and solubility \*. If this description is well drawn up, the error respecting the name may be rectified, and the observation will not be lost, as it would be were there any reason to suspect the justness of the denomination, and no means of correcting it by a description †.

16. When the characters of a fossil give it such a likeness to another that it is found near the limits which separate the genera or species of these two fossils, we must follow the example of Werner and his disciples, by marking that this fossil is intermediary, or forms a transition from the one species to the other. For if we should ascribe it exclusively to the genus A, without noting the characters which bring it near to the genus B, another observer, on seeing the same fossil, might refer it to the genus B, and no one could know which of them was deceived.

17. People are often deceived also by mixing opinion with observation, and giving the former for the latter; as when people assert, that they have seen vestiges of extinguished volcanoes, because they have seen black or porous stones, or stones of a prismatic form, without deigning to describe them with care, but by qualifying them merely as *lava* or *basaltes*.

18. In the last place, a very frequent source of error is, too great a confidence in the fidelity of one's memory, or in the justness of one's first observations. These two kinds of confidence go often hand in hand; and people cannot guard against the errors, which are the consequence of them, but by noting down, on the spot, all observations to which any importance is attached, especially if they are a little complex, and carry away specimens, with their characters carefully marked upon them, of the objects that are the subject of

\* Hardness, refrangibility, electricity, &c. II.

† A person now dead, who in his time was considered as a mineralogist, wrote to me that he had found marine shells in granite. I begged him to give me an exact description of the stone which he called *granite*. He did so; but I perceived that the stone was a free-stone or sand-stone, and the specimens he afterwards sent me proved that I was not deceived. We may here recollect Recupero's pyrites of Atna. The errors of this kind, arising from false denominations, are innumerable; for an exact knowledge of mineral substances is more difficult to be obtained, and more rare, than is generally imagined.—Note of the AUTHOR

these observations; for it is not specimens of rare objects merely that should be collected. The end, indeed, of the geological observer is, not to form a cabinet of curiosities, but he must carry away fragments of things apparently the most common, when an exact determination of their nature may be interesting to theory. People may thus employ, with advantage, the means of confirming or rectifying their first observations, and of making profound researches and comparisons impossible to be made on the spot \*.

[To be continued.]

*V. Observations on Animal Electricity, and particularly that called Spontaneous. By J. J. HEMMER.*

[Concluded from Page 7.]

**I**N a letter, dated June 21, 1787, which I received from M. De Saussure, he confesses that he had not made any farther experiments on animal electricity; and that he did not know whether any had been made by others. As I was convinced, however, that a complete knowledge of that electricity which is produced in the human body by the friction of the clothes, as well as spontaneously, might be of great

\* We think it our duty to subjoin here some advice to travellers in regard to the questions which they may ask in the different towns.

Whence do they procure the materials proper for building; such as lime, plaster, tiles, slate, stones of different kinds, and sand? Do they burn turf or coal; and where are they found? Where do they procure their potter's clay, fuller's earth, the clay used for refining sugar, their whetstones and millstones? To observe with what the streets are paved; of what stone the steps of stairs are formed; marks for boundaries, &c.; and to learn from what place they are brought. To ascertain whether wells or the foundation of houses are dug; and whether there are in the neighbourhood any ravines or precipices. These questions will serve to facilitate the means of observing the nature of the ground, by pointing out the natural or artificial excavations that may exist in the neighbourhood, or which ought to be visited. For the same reason it is proper to examine the shores of rivers. It will be of use also to take a general view of the country from the tops of towers and of the highest steeples. It will be of some importance also to enquire, in the country, whether the inhabitants make use of lime, marl, plaster, coal, earth, or turf-ashes, for manuring their land; and from what places these substances are procured. C.

utility

utility both to the science of electricity in general, and particularly to medical electricity, I resolved to make researches on this subject by means of experiments. The method I employed was as follows:—In order that I might examine the electricity of my own body, I insulated myself on a board which stood on glass feet. I then touched, for a determined time, (at first commonly half a minute, afterwards only a moment,) the plate of my condenser, which I have described in another place\*; I then applied the condenser to a Cavallo's highly sensible electrometer, as improved by M. De Saussure, and, by means of a glass tube rubbed with a piece of woollen cloth, examined the kind of electricity when announced by the diverging threads. The electricity of the plate of the condenser or electrometer, which corresponded with that of the body, I marked with + when positive, with — when negative, and with 0 when none was present. I made my experiments on the 21st of February 1786; and since that period have repeated them, not only on myself, but on other persons, both male and female, of various ages and different constitutions, when they were in motion, or at rest; dressed and undressed; when tired, and in good spirits; hot or cold; fasting or full; sleeping or awake; at different temperatures of the weather and of the apartment, &c.

### *Results of the Experiments.*

1. Animal electricity is common to all men; for, as I found it, in the course of my experiments, on thirty persons of both sexes and all ages, and of every habit of body, I can with justice conclude on its being general.

2. Animal electricity is different in different men, at the same place and at the same time; not only in strength, but also in the kind; being in many weak, and others strong; in some positive, and in some negative.

3. This difference of electricity is often observed in different persons, when, besides time and place, the other circumstances appear to be all equal.

4. The strength and kind of this electricity are often dif-

\* See *Gren's Journal der Physik*, Vol. II. Part II. p. 210.

ferent;

ferent; not only in different men, but also in the same person. In 2422 experiments, in which I examined my own electricity, I found it 1252 times positive, 771 times negative, and 399 times 0. The electricity of my maid, in 94 experiments, was, on the other hand, 19 times positive, 33 times negative, and 42 times 0.

5. It often happens, that, during the experiment, the electricity changes; that is, from positive to negative, or from stronger positive to weaker, and at last becomes 0. It then passes from the last to weak negative, and gradually to stronger.

6. It happens also, not unfrequently, that the electricity, at the commencement of the experiment, is strongly positive or negative, but passes afterwards to the opposite kind with the like force, which continues for a considerable time.

7. Notwithstanding this wonderful and almost continual alternation, animal electricity appears to be naturally positive. As the electric matter is diffused throughout all nature, and adheres, in a fixed form, to no body, all the articles of our nourishment contain each their share of it. The electric fluid is disengaged, and diffused with the blood and juices through the body; from which it is conveyed off, after being accumulated in it, by the pores of the skin and other ducts, when there is no interruption in them. This is confirmed by experience; for when animal electricity is examined in a body exposed to no violent exertions, as when a person sits or reposes, and when there is consequently no great loss of heat, it is commonly positive. Thus the electricity of my body, which I examined while sitting at rest, when the natural heat of the body was not disturbed, appeared, in 332 experiments positive, in 14 negative, and in 10 was 0.

8. When animal electricity, therefore, is 0, or negative, the body must be in an unnatural state (*statu violento*).

9. Cold changes natural or positive animal electricity into the opposite kind, or at least lessens it. In 60 experiments I made on myself after coming from a cold air, the temperature of which was at the freezing point or below it, my electricity was 38 times negative, 7 times 0, and 15 times positive, the last being not unfrequently weak. I had often



an opportunity of observing the pleasant phenomenon, that the negative electricity arising from cold, after pulling off a cold and putting on a warm coat, was speedily converted into positive, or the degrees next to it. When the thermometer stood several degrees above the freezing point, and a somewhat cold wind prevailed, the latter was sufficient to produce negative or 0 electricity, as I experienced four times. Of 13 times, while I sat lightly clothed in a warm apartment, the electricity was 8 times 0, thrice—, and twice weakly +. Nay, when I clothed myself well, and remained any time at a cooler part of the room, such as the open door or window, I found the electricity either negative or weakly positive. For producing this effect, it is in general sufficient if a part only of the warm body be exposed to cold. When the electricity was commonly +, while I sat at perfect rest, on my usual chair, I oftentimes remarked that it was perceptibly negative if my feet were extremely cold, without being able to assign any other cause for this phenomenon. It is, however, fully proved by the following circumstance: During 23 times that I washed my hands and face with cold water, the electricity was only 15 times perceptibly +, 8 times —, thrice 0, and 7 times weakly +; though a moment before, or after, it was, for the most part, strongly +.

That cold impedes positive electricity, or makes it less and reduces it to 0, may be easily comprehended; because cold causes every thing to contract, lessens the pores of animal bodies, and, by these means, obstructs the escape of the electric matter. That it, however, converts this electricity into negative, or gives to the external part of animal bodies the property of extracting from other bodies, when in contact with them, a part of their natural electricity, is not easy to be explained; for, in order to do so, they must themselves have sustained a loss of their natural quantity of electricity. But when, by the contraction of the pores, they receive no more of the electric matter from the interior parts of the body, they must, in order that the equilibrium be kept up, not sustain any loss of it. This loss is either real or apparent. The former can scarcely be asserted, as there are not sufficient grounds for such an idea. For if it be said that the electric matter

matter naturally contained in the external part of the body, is expelled by that accumulated more and more in the interior parts, on account of the cold, and which thereby acquires a stronger repulsive power, it is of no weight; since this power would prevent the plate of the condenser from giving up any of its natural electricity to the finger when applied to it. There is more reason, therefore, to assert the latter, *viz.* that this want is only apparent; that is, that the electric fluid, which the external parts of the body naturally possess, is not expelled by the effects of cold, but fixed, or so fast united with the parts in which it is contained, that it loses all movement, and gives no indications of its presence. This kind of union is not uncommon in nature: it takes place in regard to the particles of fire, which produce heat; when moist bodies evaporate, or when salt is mixed with ice.

10. Lassitude impedes also the positive animal electricity, or changes it into the opposite. During 16 times that I walked backwards and forwards in my apartment, or was otherwise employed, the temperature of the weather being 10, 12, 14, and more degrees of heat, and no cold wind prevailing at the time, I found the electricity only once weakly +, five times 0, and ten times —. In 32 experiments, when standing at rest, the electricity was 30 times 0, and twice weakly +. In 27 experiments, while I sat at rest, it was always strongly +; and in five experiments, while walking at a moderate pace, it was perceptibly +. I found the case to be the same on my servant and another young man.

I will not, however, say that the electricity, when the body is in a state of rest, always ceases, or is changed from positive. Many times the strength of the body is greater than to allow of its being exhausted by fatigue, as I have more than once experienced.

11. By rubbing a part of the body, positive electricity is not impeded. The rubbing may be performed with linen or woollen.

12. Sudden, speedy, and violent motion can change every kind of animal electricity into the opposite. A luminous and beautiful appearance was occasioned when I inclined and raised my body suddenly in turns, and threw upwards quickly  
and

and with a certain degree of violence, sometimes my arms and sometimes my legs. Under these circumstances the electricity was thrice changed from — to +, and 16 times from 0, or weak +, to —. On my servant, under the same circumstances, it was eight times changed from + to —; and on my maid five times from — to +.

13. Every other motion of the body, not connected with such violence, or with uncommon agitation of the limbs, when it does not produce much fatigue, does not impede positive animal electricity.

14. Such motion is no impediment also to negative animal electricity.

15. After noon, or dinner-time, animal electricity is not greater than common.

16. The use of coffee makes no change in animal electricity.

17. Continual straining of the thoughts is not only favourable to positive animal electricity, but increases it in an uncommon degree.

18. Animal electricity is impeded by perspiration.

19. Animal electricity is stronger in winter than in summer.

20. Repose at noon, or any other short sleep when sitting in the day-time, does not disturb positive animal electricity.

21. The breath does not conduct animal electricity in a perceptible manner. I breathed on the cover of the condenser in various places, but I never found the least traces of electricity. The electricity, in all probability, was dispersed too speedily, or was too subtle to be perceptible.

22. Bodily motion is by no means necessary for producing animal electricity;

23. Neither does it depend on the movement occasioned by respiration.

24. Animal electricity arises also without friction of the clothes. My experiments leave no doubt on this subject; as I found the electricity on my own body lively and durable for half an hour, or an hour, when I had on no clothes. I do not say, however, that the friction of the clothes does not increase it.

25. Animal electricity is excited without any friction of the external parts of the body.

26. There is also a spontaneous kind of animal electricity.

VI. *Observations on Pot-ash; being an Inquiry how far the mischievous Effects of Septic Acid are restrained by Pot-ash and other Alkalies, particularly in respect to the Effects of Septite of Pot-ash (Nitre or Saltpetre) upon Animal Flesh intended to be eaten, and upon the Human Stomach; in a Letter from Dr. Mitchill to Dr. Priestley, dated Plandome, May 4, 1799. Communicated by Dr. MITCHILL.*

ON a former occasion (2 Medical Repository, p. 236 *et seq.*) an attempt was made to shew that the *septic acid*, which is formed in certain putrefactive processes, was materially different in its constitution and qualities from the *nitrous acid* obtained by distillation from saltpetre. Since the composition of that piece, I have observed that Juncker, in his view of the doctrines of Beccher and Stahl, (2 Conspectus Chimie, p. 280,) is of the same opinion, declaring, that in whatever manner the work of separating it from the putrescent or other bodies with which it was naturally mingled, was undertaken, “*ne micula tamen acidi nitrosi pure sistitur,*” *not a particle of pure nitrous acid can be obtained.* And he warns his reader, that in the observations which he offers he means that *spirit of nitre ONLY* which is liberated from its connection with an alkaline salt. On the native septic acid, which is, as he allows, furnished so largely by the animal kingdom, (p. 277,) and is by far the most active and interesting form of oxygenated septon, Juncker, like most other writers, has said scarcely any thing at all.

The highly destructive effects of this offspring of putrefaction have been pointed out in detail in a former essay (1 Medical Repository, p. 39, 40), in which it was observed that certain substances, and among others pot-ash in particular, possessed a power to restrain and curb its ferocity. The neutral salt, formed by the union of this pestilential acid with the fixed alkali, is the saltpetre or nitre of the shops and of commerce. Concerning the medicinal and economical qualities of this substance, it is manifest to me that there are many mistakes yet prevalent: and they appear to be worthy of being pointed out and corrected,

The septite of pot-ash has been denominated a salt of many excellent qualities, a *sal polychrestus*; and a great physician once wished, for the good of his profession, there could be found *one* other remedy so certain and steady in curing diseases as nitre. It has likewise been termed an *antiphlogistic* remedy, good for all manner of inflammatory diseases with phlogistic density of the blood; possessing fine attenuating powers, being in nowise acrimonious; and happily calculated to withstand a putrescent state of the body. It has further been called a *refrigerant*, a *diuretic*, and a *carminative*, and employed accordingly by those prescribers who are influenced by the *nominal* efficacy of remedies.

Such are some of the superlative effects ascribed to this compound of the acid of pestilence and pot-ash: and for a considerable time after I became acquainted with the mischievous effects, wrought occasionally by the naked septic acid, I remained in the belief that the strongest of the alkalis could hold it fast, and keep it entirely harmless. It therefore did not appear to me improbable that the character of the compound of the two might, as in a multitude of other cases, be exceedingly different from that of either the constituent acid or alkali.

But latterly I have been inclined to the opinion, that pot-ash is capable of combination with oxygenated septon in different degrees; that is to say, septon, before combining with the alkali, may have been united to *more* or *less* of oxygen; and also septon, in any of its degrees of oxygenation, may be united with pot-ash in different proportions: in other words, the acid may *vary in its strength*, and likewise, on every degree of strength, may be united to the alkali *in various proportion*. The *nitrum nitratum*, described by the older chemists, is an example of pot-ash *super-saturated* with nitric acid, and, strange to tell! has been extolled for its advantageous operation in ardent fevers, accompanied with thirst and with a dry and foul tongue. I have strong reason to think that there is a disproportion between the acid and alkali in other forms of nitre; as I have known litmus-paper to be turned repeatedly reddish by a watery solution of saltpetre, the residue of a quantity which was swallowed by

mistake, and which nearly deprived a man of his life. A set of correct experiments is wanting to elucidate more completely this part of a very curious and highly important subject.

Be these things as they may, all experience shews that the connexion between septic acid and pot-ash is easy to dissolve; at least, a portion of oxygen separates very readily from the nitre. The experiment of reddening blood by mixing powdered saltpetre with it, was known to Hoffman, and, I think, fairly evinces a partial decomposition of the salt. It seems to have a similar operation upon the residue of that fluid in the flesh of slaughtered animals, and the reddening of the lean and fibrous part of meat is evidently owing to the oxygen attracted from the nitre.

But a heightening of colour is not the only effect which septite of pot-ash works upon provisions sprinkled with it. There are in many meats, especially of old animals, and of those which have been a long time salted, a *toughness* and *hardness* which render them difficult to be cut and to be chewed. The septic acid seems in some degree to be disjoined from the pot-ash, and evidently assists in decomposing, to a certain point, the vascular and fibrous structure of the meat. The quantity of nitre generally put on is small; was the proportion larger, the meat would be yet further disorganised, and be rendered more *short* and *tender*, almost even to *rottenness*. But the injurious effect of the nitre is prevented by the sea-salt commonly mingled with it, in the manner and upon the principle described in my "Observations on Soda." (2 Medical Repository, p. 292, *et seq.*) The use of saltpetre, then, in curing provisions, is to make them *reddish* and *tender*, and not to exercise an *antiseptic* and *hardening* power, as the muriate of soda does.

With all these considerations before me, I entertained great doubts of the truth of those fine things told over and again, and copied by one writer from another, about the mildness and wholesomeness of nitre, and of its wonderful effects as a calmer and soother of diseases. It seemed very strange to me, that the acid which before its union with pot-ash was capable of causing fevers, should be so suddenly transformed,

transformed, as, notwithstanding the laxity of their cohesion, to cure such diseases immediately afterwards.

While I was considering these things, a case fell under my observation, which allowed me fully to witness the operation of this boasted *cooler, carminative, and febrifuge*. It shews, beyond a doubt, that septic acid, though coerced by pot-ash, is in some degree septic acid still. Its native virulence does not even then wholly forsake it. Nitre ought to be ranked among the poisons; for, in a sufficient dose, it is truly a poison. Though it may be administered in small quantities without exciting terrible symptoms, it resembles in that respect most other venomous substances, which can be swallowed without detriment, and even often with advantage, when their dose is not too large. It is time for writers of Dispensatories, and on *Materia Medica*, to know the facts concerning nitre and its operation, and to publish them for the sake of undeceiving their readers. Too long has the medical world been the dupe of idle and partial opinions on this subject. Read the following accident, and judge of what I have said from the symptoms induced by swallowing septite of pot-ash or saltpetre:—

A carman, of middle age, had followed his business as usual on Saturday the 20th day of April 1799. On Sunday morning he determined to take a saline cathartic for an indisposition too trifling to require the advice of a physician. Accordingly his wife dissolved an ounce of what she believed to be sulphate of soda (Glauber's salt) in water, and gave it to him at a draught. He swallowed it. Soon after he was severely incommoded by what he termed "a great weakness about his heart." Nausea came on, and was followed by vomiting of the contents of the stomach, mixed with considerable quantities of blood at each time. His strength was exceedingly impaired: and a sensation of coldness over the whole body was remarked by him, particularly in the extremities. I did not see him until after the bloody vomiting had continued at times for several hours, and then his pulses at the wrist were very slow. He was however quite rational, and said he then felt much better than he had done. On examining what kind of salt he had taken, for some crystals of it remained in a wide-mouthed bottle, I found it to be septite

tite of pot-ash (saltpetre.) This was about eleven o'clock before noon, and he had taken it at six in the morning. As it had had no purgative effect, I ordered him some castor-oil, and almond milk sweetened with sugar, or some milk-whey, and some water-gruel.

In 35 Commentar. de Rebus, &c. p. 176, a case is mentioned of death from taking an ounce of nitre: and if a part of the quantity which this man took had not been vomited tip, he probably would have died too. The ol. ricini purged him gently, and he gradually got better, but complained very much of weakness about the præcordia.

There are numerous other accidents not materially unlike these. Such occurrences give us no very favourable account of the benignity of nitre as a medicine. It is a pity that practisers of physic do not better understand the component parts of their prescriptions. How few know that, in administering nitre, their patients are made to swallow a portion of the nauseating and sickening acid of putrefaction!

Possibly these remarks may have a tendency to remove the doubt contained in your letter of April 11, 1799. You will hereby perceive that *my native acid of septon* is a combination of this basis with oxygen and water; whereas your *artificial acid of nitre* undergoes a partial decomposition by the heat of distillation, and is adulterated besides with whatever happens to be mingled with it during and after its combination with the vegetable fixed alkali. And both these forms of acid differ from atmospherical air; inasmuch as the former are *chemical* mixtures, the latter is *mechanical*.

It would be better for science if the word "nitre" was rejected altogether from use. *Nitria*, whence the term comes, was, you know, a district of ancient Egypt, famous for the quantity of mineral alkali which it afforded. (D'Anville's Geograph.—Egypt.) This saline substance has thence been called by the names *nitrum*, *nitre*, &c. In confirmation of which, I observe, in the Dictionary of Calepinus, printed at Basil in 1538; that what they called *nitrum* was a material employed to cleanse clothes, and wash the bodies which wore them. And S. Bochart remarks, (1 Opera. Chanan, L. II. cap. xiv.) that the ancients made a kind of ley from *ashes*,  
*soda*,



*soda*, and *bole*, (*cinere, nitro, et cimoliâ*,) for more effectually clearing their bodies from nastiness when they bathed. I need not remark to you, that I employ the word “nitre,” not in its ancient, but modern sense.

It is highly desirable that some of our men, whose opinions have weight with the public, would peruse the work of Lancisi, physician to Pope Clement XI, on the noxious exhalations of marshes. (*De Nox. Palud. Effluviis*.) By the persevering and luminous researches of this great man, it was found, as long ago as the year 1716, that marsh-water, by simple distillation, (*Ibid. Lib. II. c. xii.*) manifested an *acid* quality; and that *calcareous* stones (*Ibid. Lib. II. c. ii.*) were better for paving the streets of cities than *siliceous* ones, because the *alkaline nature* of the former was adapted to imbibe the noxious moisture of the air, and sweeten the *acid salts* with which it abounded. Indeed, much of the matter detailed by the writers of our days on local sources of distempers, may be found better observed, and better stated, by Lancisi, than in their writings. By the by I observe he mentions the English philosopher Mayow, (*Ibid. Lib. II, P. ii. cap. 2.*)

If the philosopher of Rome had reasoned upon his own discovery, he could not have failed to draw the inference, that by *alkaline substances* might the HYDRA of pestilence be overcome.—As I have mentioned this monster, I shall pause a little to give you my opinion of the allegory among the ancients concerning her: and I attempt the explanation the more willingly, as I believe Lord Verulam has said nothing about it. The fable is this:—In Peloponnesus, between Mycenæ and Argos, there was a fen or marsh of some extent called Lerna. This muddy and stagnating pool was inhabited by Hydra, a horrible and devouring monster with several heads; some say seven, others nine, and others fifty. The malignity of her poison was such that a wound from an arrow dipped into it was instantly mortal. She made dreadful havoc among the people of the surrounding country, and devoured a great number of their sheep and other cattle. In obedience to the orders of the tyrant Eurystheus, Hercules went to fight this destructive and formidable creature. On  
his

his approach, a crab came forth to the assistance of Hydra: but Hercules crushed the crab, and afterwards slew Hydra. Of the heads of Hydra, it was reported, when one was cut off, two would sprout from the wound, unless prevented by the immediate application of fire. Hercules, availing himself of the aid of fire, succeeded in his undertaking. In the ninth figure of Montfaucon's 66th plate, there is a figure of Hercules with crabs near his feet, having, as the learned father curiously enough remarks, a relation to some mystery which he does not comprehend. (1 Antiquity explained, Art. Hercules, chap. ix.)

Now, it appears to me, this is an allegory expressive of the pestilential vapours emitted by the bog of Lerna, and of the means found by experience useful to drain off its stagnant water, and to clear the adjoining and surrounding morasses.

The word "hydra" is derived from ὑδρῶς, *water*. This fluid then, detained upon the marsh of Lerna, favoured occasionally the production of unwholesome exhalations. Such vapours, being at once invisible and injurious, were ascribed to some preternatural enemy or destructive monster; and being diffused, or wasted around the country, and oftentimes cutting off both man and beast, were fancied to be the effect of the supposed monster's poison. According to their extent and virulence was she reported to have fewer or more heads for preparing and inflicting this poison. The mere draining off the water, and leaving the mud and slime bare, was termed cutting off an head; and the increase of deleterious gases, in consequence of exposing such a naked surface, was aptly expressed by the sprouting forth of two in its place. By cauterising, or searing, was understood either the solar heat in drying the ground after the water was drained away, or the burning up of the trees, shrubs, and obstacles to free ventilation by ordinary combustion, or perhaps both. The crab, who was Hydra's ally, perhaps does not refer to the sun's place in the constellation Cancer, so much as to shew the frequent recurrence of the difficulties, and the superior strength and skill requisite to overcome them. In the whole allegory "Hercules" may be understood to mean "insuperable courage and industry." North America, at the close of the 18th century,

century, wants a Hercules. This interpretation is confirmed by another consideration, that the ancients had not only their *Hydra*, who lived in the water, but their *Cherfydra*, who remained after the marsh or fen was dried up. *Cherfydra*, being derived from the two words, *χερσος*, land not fit for the plough; and *υδρξ*, the monster of the fens; will thus mean the venomous and sickly condition of the neighbouring atmosphere after the water was exhaled, and the ground at the same time not rendered arable thereby, typified by a poisonous serpent: and was thus expressive of the rage of pestilential effluvia, which sometimes, and under certain circumstances, continue in a virulent state, in dry weather, near their dried sources.

Hydra is seemingly mentioned by Virgil (*Æn.* vi. v. 576.) as a *fictitious* or *poetical* animal. Bochart, however, with his usual prodigious erudition, appears desirous to make the whole story *literally* intelligible. (*Hierozoici pars poster. Lib. III. cap. xiii.*) But Lancisi, with a more clear and discriminating mind, perceives that important physical truths are concealed under this two-fold allegory, and shews how they are to be unriddled. (*De Nox. Palud. Effl. Lib. II. p. ii. cap. 3.*)—In considering these matters you will not fail to recollect that the classical writers, and others, use the word “hydrus” as well as “hydra,” and some of them apparently confound the two. The former noun of the masculine gender is probably the name of the *real* animal, the *water-snake*, the latter of the *imaginary* one.—But of this enough.

If pestilential matter, as I observed before, can be subdued by alkalies, then the formation of septite of pot-ash in the alimentary canal must be a very frequent and common process. It is universally agreed that pestilential matter may be taken into the stomach by swallowing; and no reasonable doubt can be entertained of its production within that organ, and other parts of the intestinal tube, from the corrupted remains of food. The whole tenor of prescription, as explained in my letter concerning the use of alkaline remedies in fevers, (*1 Medical Repos. p. 265.*) goes to shew that the offending cause is of an acid quality: and in its worst forms, I think the acid engendered has septon for its basis; since aliment,

containing this principle only, is capable of yielding that most noxious compound. When septic acid thus exists in the alimentary canal, and carbonates, tartrites, &c. of pot-ash, are taken down, the stronger acid is attracted, and a proportional quantity of nitre or saltpetre is formed. Thus, in abundance of cases, the practisers who are fond of prescribing septite of pot-ash might spare themselves the trouble, as that saline compound is, in certain cases, formed in the stomach and bowels in considerable quantity. What precise effect the *nitre so formed* has on the intestines, and on the constitution at large, deserves to be inquired into with great care; for I am not without apprehension that some of the sad symptoms, occasionally attendant on fevers, are modified by the septite of pot-ash formed within the belly.—I believe, however, I must now stop, and trouble you no further for the present than by assuring you, that I hope much from your aid in this inquiry, which you inform me you consider “as a happy train of discovery;” and that I am, very respectfully, yours,

SAM. L. MITCHILL,

*To Dr. Priestley.*

VII. *Description of a Mercurial Gazometer constructed by Mr. W. H. PEPYS jun. Communicated by the Author.*

THE difficulty attending the exhibition of acid and alkaline gases, was the first inducement to Priestley, Lavoisier, and others, to use mercury for such experiments; but the great expence and enormous weight of this fluid obliged many accurate and experienced chemists to relinquish them almost entirely, as there appeared no other likely means of succeeding but by its means and that of the exhausted receiver. A contrivance to lessen the quantity of mercury necessary for such experiments was therefore a desirable object; and by introducing the dome used by Mr. Watt in his hydraulic bellows, I have succeeded in constructing an apparatus in which both of the above-mentioned requisites are obtained at a comparatively small expence. As it is hardly possible

possible to japan tin or copper so perfectly as to prevent their being attacked by mercury when brought in contact with them, I had the cylinders turned in lignum vitæ, on which the mercury has no action : the conducting tube is of glass ; and the cocks are coated on the inside with varnish.

The usefulness of an apparatus so constructed will appear sufficiently obvious, when it is recollected that all gases passed through any other fluid than mercury, water for instance, take up a quantity of moisture, which adds considerably to their gravity, and makes it impossible to determine their real weight.

For weighing of gases I make use of a glass globe and stop-cock of a smaller size than is commonly employed, as greater accuracy can be obtained by using a proportionally delicate beam than by employing a larger globe, which must be suspended to a beam of such strength as greatly to increase the friction on its axes. It is of great importance, in the analysis of bodies, or in other chemical experiments, to be able to ascertain with accuracy the weight of any gas obtained by the process. The weight of two or more quantities of gas should however be always tried, and the mean be taken to prevent any error.

A, (Plate III.) is a representation of the bell of the gazometer, made of glass, furnished with a cock at top, and able to contain 34 ounces troy of distilled water. The divisions of capacity, determined by actual measurement, are marked on the glass with a diamond. BB, section of two cylinders of lignum vitæ, the outward one screwed upon the solid internal one, which is made to project at its lower extremity, and furnished with a male screw, to work into a female screw with which the lower end of the external cylinder is furnished. The space between these is so adjusted as to be almost filled up by the substance of the glass bell A when dropped into it, so that the quantity of mercury necessary to fill up that space is proportionally small. The internal cylinder has a conducting tube up through its axis, the lower end of which is furnished with a female screw answering to the male screw of the cock of the small receiver C. The receiver C is made of glass, and open at bottom. When this

receiver is used, it is screwed into its place, and rests upon a small cup or cistern of mercury D, in which the beak of a retort, furnished with a bent glass tube, to be afterwards noticed, may be introduced under the receiver. E, E, E, E, section of a wooden stand upon which the cylinders of lignum vitæ are supported, having an opening through the top to permit the cock of the receiver C to be joined to the conducting tube of the internal cylinder B. The cistern D is adjusted to its height by means of a rising cylinder in the pedestal F. G is a transfer glass for mixing alkaline gases in vacuo, or other purposes; and, when used, is joined to the top of the bell A. H, a glass globe and stop-cock, capable of holding 14 ounces troy of distilled water for weighing gases: it receives its gas by being inverted, and screwed into the bell A. I, a bladder furnished with a stop-cock to assist in holding, transferring, or mixing different gases. K, an elastic gum-bottle, capable of containing 30 ounces of distilled water, for holding the acid gases: when used, it is screwed into the top of the transfer G; the bottom cock of the latter being at the same time joined to the bell A, previously charged with the alkaline gas: the cocks being turned, the gases rush together in vacuo. L, a small portable air-pump, for exhausting the globe H. M, a double male screw, which fits any part of the apparatus, and on which a valve may be fastened. N, a double female screw. O, a small instrument, which I would not have mentioned, had I not found it of peculiar service in collecting spilled mercury: it is made of glass, the mouth being applied at *a*: you may collect any small globules of mercury by the small end *b*, by which means they are elevated into the receiver at Q. It is useful likewise for removing mercury from the small cistern.

One of the principal objections to the use of mercury in such experiments as this apparatus is intended for, has been, the great force necessary to overcome the resistance of a column of mercury when gases are to be received over that dense fluid; a resistance in the proportion of one inch of mercury to fourteen inches of water, and which very few lutes are able to withstand. This resistance I overcome by a very simple contrivance: a bent tube fitted into the beak of

of the retort, (if one be employed,) or into a Wolf's apparatus, and passing into the upper part of the small receiver, as expressed in the plate at C. By employing mercury for such experiments, another advantage is gained by the use of this apparatus, namely, a power of exhaustion in the retort, or Wolf's bottle, equal to a column of two inches of mercury, or 28 inches of water. This will be easily conceived when it is recollected that, by drawing up the large receiver A, the small one C is raised in its cistern, bearing up with it the contained mercury, which is kept in its place by the pressure of the atmosphere on the surface of the mercury in the cistern. The cock of the small receiver C is then to be turned off, and that of the large one A to be turned on. The air, of which the retort, or Wolf's bottle, is thus exhausted, may then be let out, by plunging A into the mercury between the cylinders BB, and turning off the cock. When a sufficient quantity of gas passes from the retort, or bottle, through the bent tube into C, to level the mercury in it and the cistern, the communication may again be opened, and the same steps followed as before described. By this means I have been enabled to obtain more gas, from the same materials, than if I had received it through a fluid of the weight of water; a circumstance of some importance where nice and accurate results are looked for.

The plate of the apparatus is on a scale of nearly three inches to a foot.

VIII. *A Communication respecting the Preparation of Writing Ink. By Mr. DESORMEAUX junior.*

MR. EDITOR,

**B**EING a constant reader of your truly instructive and entertaining Work, and having noticed in the Number for October, p. 29, a communication relative to an improved writing ink, I am, in consequence, induced to offer some practical remarks on that subject.

Previous to my reading that communication respecting the discoveries of Proust having been applied by Van Mons

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to the preparation of the fluid alluded to, I had made a great variety of experiments, in some of which I treated the sulphat of iron precisely as there directed; but since that time, I have, with a view to improvement, followed the rule which is there prescribed strictly, and have besides, at former intervals, gone over and varied the experiments of Macquer, Lewis, Berthollet, Ribancourt, Proust, and other celebrated chemists, both foreign and native, who have treated on this and other subjects nearly allied to it; with a view, if possible, to establish certain data upon which to ground the best mode of fabricating the article in question, being very desirous constantly to obtain an ink for the common purposes of penmanship, which, at the same time that it should flow with freedom, should have the proper degree of lustre or glossiness, and be at the moment of using intensely black, with a capability of retaining that colour continually, even though openly exposed to the action of the sun and air. This, I presume, I have accomplished: at least, during a space of ten years, I have never met with any formula which has so well rewarded my pains as the one which I am about to communicate. Having been brought up in a line of business in which I am in the daily habit of observing the action of such substances upon each other as enter into the formation of ink, it may be readily imagined that, in a practical point of view, my opportunities for the improvement of it have fallen little short of any individual whatever. If to this I add, that, since the year 1794, I have annually supplied the public with at least 17,000 gallons of ink, besides preparing powder for its occasional production, it may be inferred that I have spared no pains in studying what would best conduce to its perfectability; and after the many attempts which I have made, I have never found my expectation or that of my friends deceived, when the process has been conducted agreeably to the following directions:—In six quarts (beer measure) of water, (it does not appear of importance whether it be rain, river, or spring water,) *boil* four ounces of the best Campeachy logwood, chipped very thin across the grain; (the boiling may be continued near an hour;) adding from time to time a little *boiling* water, to compensate for waste by evaporation: strain



the liquor *whilst hot*, suffer it to cool, and make up the quantity equal to five quarts by the further addition of cold water. To this *cold decoction* put one pound averdupois weight of blue galls, or 20 ozs. of the best galls in sorts, which should be first coarsely bruised; 4 ozs. of sulphat of iron, calcined to whiteness;  $\frac{1}{2}$  oz. of the acetite of copper, which should be triturated in a mortar, moistened by a little of the decoction gradually added till it be brought to the form of a smooth paste, and then thoroughly intermixed with the whole mass. Three ounces of coarse brown sugar, and six ounces of good gum Senegal, or Arabic, are also to be added. These several ingredients may be introduced one after the other immediately, contrary to the advice of some, who recommend the gum, &c. to be added when the ink is nearly made; as gum, however, is at present exorbitantly dear, three or four ounces will be found sufficient, with only one and an half ounce of sugar, unless, for particular purposes, it is wanted to bear a higher gloss than common. In regard to the use of sugar, which I have here recommended, I hope I shall not trespass in remarking, that my observations, on a very extensive scale, are coincident with those of M. Ribancourt, who says, that a degree of fluidity is by its means imparted, which allows the dose of gum to be enlarged considerably beyond what it would bear without it; and it is thereby rendered less liable to clog the pen, which, especially when the nib is very fine, if it does not flow freely, quits it so slow as unpleasantly to retard the writer, and is totally unfit for fine manuscripts; besides, by such an union of gum and sugar, a degree of consistency is given to the liquor, which enables it to suspend a much greater portion of colouring matter than otherwise could be effected; a circumstance of the greatest importance to its hue and permanency. It is far from my design, in thus transmitting my ideas, to induce a supposition that it is *exactly* after this manner that I, or any other person who supply ink by wholesale, compound it; so far from it, that the first cost of one gallon, on the plan here suggested, would exceed twice the sum which is usually paid for it to the manufacturer, although by retail it is disposed of at an enormous rate. For private persons, therefore, who wish to  
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be particular in their manuscripts, the rule I have here laid down will be found fully to answer their expectations at a cheaper rate than they are in general furnished with it. The best vessel, in my opinion, in which to make ink, is a common stone bottle, which will contain half as much more as is proposed to be made, and which should be agitated twice a day, and be left uncorked, in order to expose fresh surfaces of the liquor to the action of the air, without which it cannot be expected to write very black when first used, but with this precaution may be fit for use in about 14 days; when it may be poured from its dregs, and corked up, to preserve it from dust, as well as waste by evaporation. In cold weather it should never be suffered to freeze, which is found to cause a disunion of its parts, nor does it ever afterwards recover its former intensity of colour, lustre, or durability. Many persons, I am aware, disapprove, and therefore omit the use of logwood, from an apprehension that it induces a tendency in the ink to fade; this however, from the strictest attention, I can assert to be fallacious, at least where it is not had recourse to by way of substitute for the other more expensive articles; indeed its effects are quite the reverse, where all the ingredients are properly proportioned. As to the beautiful complexion which is given by it to the entire mass of fluid, it would be needless to insist, unless for the sake of those who are unacquainted how a mixture of the acetite of copper and logwood liquor work together, and by which, in this instance, a richness and bloom is given, which can in no other way be got with equal economy and success; independent of which, the colouring matter of the wood, by its affinity with the oxyde of iron, has a very powerful effect to blacken the ink, and to render it less capable of change from any unsaturated acid in the sulphat of iron, or from the operation of the air. I am aware, too, that the introduction of cupreous matter has been objected to, on the score of its injuring the penknife by a portion of copper attaching itself thereto in consequence of superior elective attraction, whereby it has been remarked that a part of the knife's edge is dissolved, and a quantity of copper answering thereto is exchanged and deposited in its place, which is always said to happen when the pen retains  
a little

a little ink at the time of its being mended. This, I think, is spinning out the effect of chemical affinity too far; for although, with respect to the reasoning, the truth must be admitted, I contend, notwithstanding, that its consequences to the edge are not apparent to the eye; nor is any sensible detrimental change thereby produced upon the knife, more than what is induced by any other ink into which neither the sulphat or the acetite of copper is introduced. As to myself, however, I have never yet seen a receipt for the formation of ink, where each of the principles were so completely proportioned and saturated with each other as to be inert when applied to the surface of a piece of polished metal, such as iron, for example; nay, even the frequent contact of water only, if allowed to remain, would spoil any instrument made of that material. I shall not enlarge on this particular, conceiving the most powerful objection I can offer in opposition to that opinion, is, that I have now lying by me a penknife, which has been in constant use between two and three years, and which even yet retains almost as good an edge as it had at first, by the mere assistance of an unprepared leather strap, although in the ink with which I write the acetite of copper has always been used. But even admitting, for argument's sake, that what I have attempted to disprove is true, how many hundred pens might be made and mended ere it would be requisite to bestow twopence to the cutler for the exercise of his art? In short, if the introduction of the substance alluded to into ink be a fault, I consider it as one of no consequence when put in competition with the advantages of beauty, durability, and intensity of colour imparted by it.

I shall intrude no longer on the time or patience of your readers, than by requesting, if any of them are possessed of a cheap and efficacious method whereby the mouldiness of ink may be prevented, that they will communicate the same through the medium of your Magazine, in doing which a very considerable service will be rendered towards its perfectability. The admixture either of a small quantity of the muriate of soda, of the nitrate of pot-ash, of alcohol, or of cloves, have been severally recommended for this purpose; but by experience I know that of these only alcohol will

VOL. V. Y avail,

avail, and this cannot be added in sufficient quantity without causing it to sink into, and spread upon the paper; so that, indeed, *its* use is altogether interdicted. The best plan which I have hitherto found as a preventive to the vegetative process I here allude to, is, to add the ingredients of which the ink is composed to the *cold* decoction; if recourse be had to boiling, *ALL together*, it is found very rapidly to promote the inconvenience; and were ebullition totally avoided, it would on that account be better, but then we could not obtain the ink of so deep a colour; yet I know of no instance in which mouldiness will not appear in time, and that in no inconsiderable degree. In fine, I am ignorant of any substance, or method, that has been hitherto used as a specific to remedy the defect. If there is any that can be suggested, I should be happy to put it to the test of experiment.

I am, Sir, yours, &c.

L. DESORMEAUX jun.

No. 8, Vine Court, Spitalfields.

IX. *An Inquiry concerning the Weight ascribed to Heat.* By BENJAMIN COUNT RUMFORD, F.R.S.M.R.I.A. &c,  
*Read before the Royal Society May 2, 1799.*

THE various experiments which have hitherto been made with a view to determine the question so long agitated, relative to the weight which has been supposed to be gained, or to be lost, by bodies upon their being heated, are of a nature so very delicate, and are liable to so many errors, not only on account of the imperfections of the instruments made use of, but also of those, much more difficult to appreciate, arising from the vertical currents in the atmosphere, caused by the hot or the cold body which is placed in the balance, that it is not at all surprising that opinions have been so much divided relative to a fact so very difficult to ascertain.

It is a considerable time since I first began to meditate on this subject, and I have made many experiments with a view to its investigation; and in these experiments I have taken  
all

all those precautions to avoid errors, which a knowledge of the various sources of them, and an earnest desire to determine a fact which I conceived to be of importance to be known, could inspire: but though all my researches tended to convince me, more and more, that *a body acquires no additional weight upon being heated*, or rather, that heat has no effect whatever upon the weights of bodies, I have been so sensible of the delicacy of the inquiry that I was for a long time afraid to form a decided opinion upon the subject.

Being much struck with the experiments recorded in the *Transactions of the Royal Society*, Vol. LXXV. made by Dr. Fordyce, upon the weight said to be acquired by water upon being frozen; and being possessed of an excellent balance, belonging to his most Serene Highness the Elector Palatine Duke of Bavaria, early in the beginning of the winter of the year 1787 (as soon as the cold was sufficiently intense for my purpose) I set about to repeat those experiments, in order to convince myself whether the very extraordinary fact related might be depended on; and with a view to removing, as far as was in my power, every source of error and deception, I proceeded in the following manner:—

Having provided a number of glass bottles, of the form and size of what in England is called a Florence flask (blown as thin as possible) and of the same shape and dimensions, I chose out from amongst them two, which, after using every method I could imagine of comparing them together, appeared to be so much alike as hardly to be distinguished.

Into one of these bottles, which I shall call A, I put 4107,86 grains troy of pure distilled water, which filled it about half full; and into the other, B, I put an equal weight of weak spirit of wine; and, sealing both the bottles hermetically, and washing them and wiping them perfectly clean and dry on the outside, I suspended them to the arms of the balance, and placed the balance in a large room, which for some weeks had been regularly heated every day by a German stove, and in which the air was kept up to the temperature of 61° of Fahrenheit's thermometer, with very little variation. Having suffered the bottles, with their contents, to remain in this situation till I conceived they

must have acquired the temperature of the circumambient air, I wiped them afresh with a very clean dry cambric handkerchief, and brought them into the most exact equilibrium possible, by attaching a small piece of very fine silver wire to the arm of the balance, to which the bottle which was the lightest was suspended.

Having suffered the apparatus to remain in this situation about twelve hours longer, and finding no alteration in the relative weights of the bottles, (they continuing all this time to be in the most perfect equilibrium,) I now removed them into a large uninhabited room fronting the north, in which the air, which was very quiet, was at the temperature of  $29^{\circ}$  F., the air without doors being at the same time at  $27^{\circ}$ ; and, going out of the room, and locking the door after me, I suffered the bottles to remain forty-eight hours, undisturbed, in this cold situation, attached to the arms of the balance as before.

At the expiration of that time I entered the room, using the utmost caution not to disturb the balance, when, to my great surprise, I found that the bottle A very sensibly preponderated.

The water which this bottle contained was completely frozen into one solid body of ice; but the spirit of wine, in the bottle B, shewed no signs of freezing.

I now very cautiously restored the equilibrium, by adding small pieces of the very fine wire of which gold lace is made, to the arm of the balance to which the bottle B was suspended, when I found that the bottle A had augmented its weight by  $\frac{1}{35904}$  part of its whole weight at the beginning of the experiment; the weight of the bottle with its contents having been 4811,23 grains troy, (the bottle weighing 703,37 grains, and the water 4107,86 grains,) and it requiring now  $\frac{134}{10000}$  parts of a grain, added to the opposite arm of the balance, to counterbalance it.

Having had occasion just at this time to write to my friend, Sir Charles Blagden, upon another subject, I added a postscript to my letter, giving him a short account of this experiment, and telling him how "*very contrary to my expectation*" the result of it had turned out: but I soon after found that I  
had

had been too hasty in my communication. Sir Charles, in his answer to my letter, expressed doubts respecting the fact; but, before his letter had reached me, I had learned, from my own experience, how very dangerous it is, in philosophical investigations, to draw conclusions from single experiments.

Having removed the balance, with the two bottles attached to it, from the cold into the warm room, (which still remained at the temperature of  $61^{\circ}$ ;) the ice in the bottle A gradually thawed; and being at length totally reduced to water, and this water having acquired the temperature of the surrounding air, the two bottles, after being wiped perfectly clean and dry, were found to weigh as at the beginning of the experiment before the water was frozen.

This experiment, being repeated, gave nearly the same result, the water appearing, when frozen, to be heavier than in its fluid state; but some irregularity in the manner in which the water lost the additional weight which it had appeared to acquire upon being frozen, when it was afterwards thawed, as also a sensible difference in the quantities of weight apparently acquired in the different experiments, led me to suspect that the experiment could not be depended on for deciding the fact in question: I therefore set about to repeat it, with some variations and improvements:—but, before I give an account of my further investigations relative to this subject, it may not be amiss to mention the method I pursued for discovering whether the appearances mentioned in the foregoing experiments might not arise from the imperfections of my balance; and it may likewise be proper to give an account, in this place, of an intermediate experiment which I made, with a view to discover, by a shorter route, and in a manner less exceptionable than that above-mentioned, whether bodies actually lose, or acquire, any weight, upon acquiring an additional quantity of latent heat.

My suspicions respecting the accuracy of the balance arose from a knowledge (which I acquired from the maker of it) of the manner in which it was constructed.

The three principal points of the balance having been determined, as nearly as possible, by measurement, the axes of

motion were firmly fixed in their places in a right line, and the beam being afterwards finished, and its two arms brought to be in equilibrio, the balance was proved by suspending weights, which before were known to be exactly equal, to the ends of its arms.

If with these weights the balance remained in equilibrio, it was considered as a proof that the beam was just; but, if one arm was found to preponderate, the other was gradually lengthened, by beating it upon an anvil, until the difference of the lengths of the arms was reduced to nothing, or until equal weights, suspended to the two arms, remained in equilibrio; care being taken, before each trial, to bring the two ends of the beam to be in equilibrio, by reducing, with the file, the arm which had been lengthened.

Though in this method of constructing balances the most perfect equality in the lengths of the arms may be obtained, and consequently the greatest possible accuracy, when used at a time when the temperature of the air is the same as when the balance was made; yet, as it may happen that, in order to bring the arms of the balance to be of the same length, one of them may be much more hammered than the other, I suspected it might be possible that the texture of the metal forming the two arms might be rendered so far different by this operation as to occasion a difference in their expansions with heat; and that this difference might occasion a sensible error in the balance, when, being charged with a great weight, it should be exposed to a considerable change of temperature.

To determine whether the apparent augmentation of weight, in the experiments above related, arose in any degree from this cause, I had only to repeat the experiment, causing the two bottles A and B to change places upon the arms of the balance; but, as I had already found a sensible difference in the results of different repetitions of the same experiment, made as nearly as possible under the same circumstances, and as it was above all things of importance to ascertain the accuracy of my balance, I preferred making a particular experiment for that purpose.

My first idea was, to suspend to the arms of the balance, by  
very



very fine wires, two equal globes of glass filled with mercury, and, suffering them to remain in my room till they should have acquired the known temperature of the air in it, to have removed them afterward into the cold, and to have seen if they still remained in equilibrio under such difference of temperature: but, considering the obstinacy with which moisture adheres to the surface of glass, and being afraid that somehow or other, notwithstanding all my precautions, one of the globes might acquire or retain more of it than the other, and that by that means its apparent weight might be increased; and having found, by a former experiment, of which I have already had the honour of communicating an account to the Royal Society, that the gilt surfaces of metals do not attract moisture; instead of the glass globes filled with mercury, I made use of two equal solid globes of brass, well gilt and burnished, which I suspended to the arms of the balance by fine gold wires.

These globes, which weighed 4975 grains each, being wiped perfectly clean, and having acquired the temperature ( $61^{\circ}$ ) of my room, in which they were exposed more than twenty-four hours, were brought into the most scrupulous equilibrium, and were then removed, attached to the arms of the balance, into a room in which the air was at the temperature of  $26^{\circ}$ , where they were left all night.

The result of this trial furnished the most satisfactory proof of the accuracy of the balance; for, upon entering the room, I found the equilibrium as perfect as at the beginning of the experiment.

Having thus removed my doubts respecting the accuracy of my balance, I now resumed my investigations relative to the augmentation of weight which fluids have been said to acquire upon being congealed.

In the experiments which I had made, I had, as I then imagined, guarded as much as possible against every source of error and deception. The bottles being of the same size, neither any occasional alteration in the pressure of the atmosphere during the experiment, nor the necessary and unavoidable difference in the densities of the air in the hot and in the cold rooms in which they were weighed, could affect  
their

their apparent weights; and their shapes and their quantities of surface being the same, and as they remained for such a considerable length of time in the heat and cold to which they were exposed, I flattered myself that the quantities of moisture remaining attached to their surfaces could not be so different as sensibly to effect the results of the experiments. But, in regard to this last circumstance, I afterwards found reason to conclude that my opinion was erroneous.

Admitting the fact stated by Dr. Fordyce, (and which my experiments had hitherto rather tended to corroborate than to contradict,) I could not conceive any other cause for the augmentation of the apparent weight of water, upon its being frozen, than the loss of so great a proportion of its latent heat as that fluid is known to evolve when it congeals; and I concluded, that if the loss of latent heat added to the weight of one body, it must of necessity produce the same effect on another, and consequently, that the augmentation of the quantity of latent heat must, in all bodies, and in all cases, diminish their apparent weights.

To determine whether this is actually the case or not, I made the following experiment:—

Having provided two bottles, as nearly alike as possible, and in all respects similar to those made use of in the experiments above-mentioned, into one of them I put 4012,46 grains of water, and into the other an equal weight of mercury; and sealing them hermetically, and suspending them to the arms of the balance, I suffered them to acquire the temperature of my room,  $61^{\circ}$ ; then, bringing them into a perfect equilibrium with each other, I removed them into a room in which the air was at the temperature of  $34^{\circ}$ , where they remained twenty-four hours. But there was not the least appearance of either of them acquiring or losing any weight.

Here it is very certain that the quantity of heat lost by the water must have been very considerably greater than that lost by the mercury, the specific quantities of latent heat in water and in mercury having been determined to be to each other as 1000 to 33; but this difference in the quantities of heat lost, produced no sensible difference on the weights of the fluids in question,

Had

Had any difference of weight really existed, had it been no more than *one millionth* part of the weight of either of the fluids, I should certainly have discovered it; and, had it amounted to so much as  $\frac{1}{750000}$  part of that weight, I should have been able to have measured it: so sensible, and so very accurate, is the balance which I used in these experiments.

I was now much confirmed in my suspicions, that the apparent augmentation of the weight of the water upon its being frozen, in the experiments before related, arose from some accidental cause; but I was not able to conceive what that cause could possibly be,—unless it were either a greater quantity of moisture attached to the external surface of the bottle which contained the water, than to the surface of that containing the spirits of wine,—or some vertical current or currents of air, caused by the bottles, or one of them, not being exactly of the temperature of the surrounding atmosphere.

Though I had foreseen, and, as I thought, guarded sufficiently against these accidents, by making use of bottles of the same size and form, and which were blown of the same kind of glass, and at the same time, and by suffering the bottles in the experiments to remain for so considerable a length of time exposed to the different degrees of heat and of cold, which alternately they were made to acquire; yet, as I did not know the relative conducting powers of ice, and of spirit of wine, with respect to heat; or, in other words, the degrees of facility or difficulty with which they acquire the temperature of the medium in which they are exposed, or the time taken up in that operation; and, consequently, was not *absolutely certain* as to the equality of the temperatures of the contents of the bottles at the time when their weights were compared, I determined now to repeat the experiments, with such variations as should put the matter in question out of all doubt.

I was the more anxious to assure myself of the real temperatures of the bottles and of their contents, as any difference in their temperatures might vitiate the experiment, not only by causing unequal currents in the air, but also by causing, at the same time, a greater or less quantity of moisture to remain attached to the glass.

To remedy these evils, and also to render the experiment more striking and satisfactory in other respects, I proceeded in the following manner:—

Having provided three bottles, A, B, and C, as nearly alike as possible, and resembling in all respects those already described; into the first, A, I put 4214,28 grains of water, and a small thermometer, made on purpose for the experiment, and suspended in the bottle in such a manner that its bulb remained in the middle of the mass of water; into the second bottle, B, I put a like weight of spirit of wine, with a like thermometer; and, into the bottle C, I put an equal weight of mercury.

These bottles, being all hermetically sealed, were placed in a large room, in a corner far removed from the doors and windows, and where the air appeared to be perfectly quiet; and, being suffered to remain in this situation more than twenty-four hours, the heat of the room ( $61^{\circ}$ ) being kept up all that time with as little variation as possible, and the contents of the bottles A and B appearing, by their inclosed thermometers, to be exactly at the same temperature, the bottles were all wiped with a very clean, dry, cambric handkerchief; and, being afterwards suffered to remain exposed to the free air of the room a couple of hours longer, in order that any inequalities in the quantities of heat, or of the moisture attached to their surfaces, which might have been occasioned by the wiping, might be corrected by the operation of the atmosphere by which they were surrounded, they were all weighed, and were brought into the most exact equilibrium with each other by means of small pieces of very fine silver wire attached to the necks of those of the bottles which were the lightest.

This being done, the bottles were all removed into a room in which the air was at  $30^{\circ}$ , where they were suffered to remain, perfectly at rest and undisturbed, forty-eight hours; the bottles A and B being suspended to the arms of the balance, and the bottle C suspended, at an equal height, to the arm of a stand constructed for that purpose, and placed as near the balance as possible, and a very sensible thermometer suspended by the side of it.

At

At the end of forty-eight hours (during which time the apparatus was left in this situation) I entered the room, opening the door very gently for fear of disturbing the balance; when I had the pleasure to find the three thermometers, *viz.* that in the bottle A, which was now inclosed in a solid cake of ice, that in the bottle B, and that suspended in the open air of the room, all standing at the same point,  $29^{\circ}$  F., and the bottles A and B *remaining in the most perfect equilibrium.*

To assure myself that the play of the balance was free, I now approached it very gently, and caused it to vibrate; and I had the satisfaction to find, not only that it moved with the utmost freedom, but also, when its vibration ceased, that it rested precisely at the point from which it had set out.

I now removed the bottle B from the balance, and put the bottle C in its place; and I found that *that* likewise remained of the same apparent weight as at the beginning of the experiment, being in the same perfect equilibrium with the bottle A as at first.

I afterwards removed the whole apparatus into a warm room, and, causing the ice in the bottle A to thaw, and suffering the three bottles to remain till they and their contents had acquired the exact temperature of the surrounding air, I wiped them very clean, and, comparing them together, I found their weights remained unaltered.

This experiment I afterwards repeated several times, and always with precisely the same result; the water, *in no instance*, appearing to gain or to lose the least weight upon being frozen, or upon being thawed; neither were the relative weights of the fluids in either of the other bottles in the least changed, by the various degrees of heat, and of cold, to which they were exposed.

If the bottles were weighed at a time when their contents were not *precisely of the same temperature*, they would frequently appear to have gained, or to have lost, something of their weights; but this doubtless arose from the vertical currents which they caused in the atmosphere upon being heated or cooled in it; or to unequal quantities of moisture attached to the surfaces of the bottles; or to both these causes operating together.

As I knew that the conducting power of mercury, with respect to heat, was considerably greater than either that of water or that of spirit of wine, while its capacity for receiving heat is much less than that of either of them, I did not think it necessary to inclose a thermometer in the bottle C, which contained the mercury; for it was evident that when the contents of the other two bottles should appear, by their thermometers, to have arrived at the temperature of the medium in which they were exposed, the contents of the bottle C could not fail to have acquired it also, and even to have arrived at it before them; for, the time taken up in the heating or in the cooling of any body, is, *cæteris paribus*, as the capacity of the body to receive and retain heat *directly*, and as its conducting power *inversely*.

The bottles were suspended to the balance by silver wires, about two inches long, with hooks at the ends of them; and, in removing and changing the bottles, I took care not to touch the glasses. I likewise avoided, upon all occasions, and particularly in the cold room, coming near the balance with my breath, or touching it, or any part of the apparatus, with my naked hands.

Having determined that water does not acquire or lose any weight upon being changed from a state of *fluidity* to that of *ice*, and *vice versa*, I shall now take my final leave of a subject which has long occupied me, and which has cost me much pains and trouble; being fully convinced, from the results of the above-mentioned experiments, that if heat be in fact a *substance*, or matter, (a fluid *sui generis*, as has been supposed,) which, passing from one body to another, and being accumulated, is the immediate cause of the phenomena we observe in heated bodies, (of which, however, I cannot help entertaining doubts,) it must be something so infinitely rare, even in its most condensed state, as to baffle all our attempts to discover its gravity. And, if the opinion which has been adopted by many of our ablest philosophers, that heat is nothing more than an intestine vibratory motion of the constituent parts of heated bodies, should be well founded, it is clear that the weights of bodies can in nowise be affected by such motion.

It is, no doubt, upon the supposition that heat is a substance distinct from the heated body, and which is accumulated in it, that all the experiments which have been undertaken with a view to determine the weight which bodies have been supposed to gain, or to lose, upon being heated or cooled, have been made; and upon this supposition (but without, however, adopting it entirely, as I do not conceive it to be sufficiently proved,) all my researches have been directed.

The experiments with *water*, and with *ice*, were made in a manner which I take to be perfectly unexceptionable; in which no foreign cause whatever could affect the results of them; and the quantity of heat which water is known to part with, upon being frozen, is so considerable, that if this loss has no effect upon its apparent weight, it may be presumed that we shall never be able to contrive an experiment by which we can render the weight of heat sensible.

Water, upon being frozen, has been found to lose a quantity of heat amounting to 140 degrees of Fahrenheit's thermometer; or, which is the same thing, the heat which a given quantity of water, previously cooled to the temperature of freezing, actually loses, upon being changed to ice, if it were to be imbibed and retained by an equal quantity of water, at the given temperature, (that of freezing,) would heat it 140 degrees, or would raise it to the temperature of  $(32^{\circ} + 140)$   $172^{\circ}$  of Fahrenheit's thermometer, which is only  $60^{\circ}$  short of that of boiling water; consequently, any given quantity of water, at the temperature of freezing, upon being actually frozen, loses almost as much heat as, added to it, would be sufficient to make it boil.

It is clear, therefore, that the difference in the quantities of heat contained by the water in its fluid state, and heated to the temperature of  $61^{\circ}$  F., and by the ice, in the experiments before-mentioned, was *at least* nearly equal to that between water in a state of boiling, and the same at the temperature of freezing.

But this quantity of heat will appear much more considerable, when we consider the great capacity of water to contain heat, and the great apparent effect which the heat that water loses upon being frozen would produce, were it to be imbibed

bibed by, or communicated to, any body whose power of receiving and retaining heat is much less.

The capacity of water to receive and retain heat, or what has been called its specific quantity of latent heat, has been found to be to that of gold as 1000 to 50, or as 20 to 1; consequently, the heat which any given quantity of water loses upon being frozen, were it to be communicated to an equal weight of gold at the temperature of freezing, the gold, instead of being heated 162 degrees, would be heated  $140 \times 20 = 2800$  degrees, or would be raised to a *bright red heat*.

It appears therefore to be clearly proved, by my experiments, that a quantity of heat equal to that which 4214 grains (or about  $9\frac{3}{4}$  oz.) of gold would require to heat it from the temperature of freezing water to be *red hot*, has no sensible effect upon a balance capable of indicating so small a variation of weight as that of  $\frac{1}{1000000}$  part of the body in question; and if the weight of gold is neither augmented nor lessened by *one millionth part*, upon being heated from the point of *freezing water* to that of a *bright red heat*, I think we may very safely conclude, that ALL ATTEMPTS TO DISCOVER ANY EFFECT OF HEAT UPON THE APPARENT WEIGHTS OF BODIES WILL BE FRUITLESS.

X. *Extract from the Report respecting Experiments made at the Polytechnic School in the Years V. and VI. on the Combustion of the Diamond.* By C. GUYTON.

[Concluded from Page 61.]

*Repetition of the Experiment in Fructidor, Year VI.*

AFTER examining what means were most proper for preventing the vessels from cracking by the inequality of their dilatation, it was found that there were none more certain than to employ, instead of a glass bell, a well-chosen globe of a moderate thickness, and of such a size that its surface might be at a sufficient distance from the point of the luminous cone.

The



The globe which we judged best for answering these conditions was 28.63 centimetres in diameter: its content was 123.25 decilitres, or 12,325 cubic centimetres, comprehending that portion of the neck which it was thought proper to retain, and which was 159 millimetres. That we might the more easily observe the rising and falling of the mercury in the inside, and thence determine the volumes of gas, we cemented on the outside slips of paper, on which we traced, by means of measured vessels, scales indicating decilitres, or or 100 cubic centimetres.

It may be readily conceived, that it was not possible to fill so frail a vessel with mercury in order to displace it afterwards by oxygen gas. In transferring the gas by means of water, we should have been obliged to leave a portion of that liquid on the interior side of the vessel. We determined, therefore, to convey the gas, at the moment when it was disengaged from the oxygenated muriat of pot-ash, by means of a tube adapted to the distilling apparatus, and made to descend to the bottom of the globe in such a manner that the common air should be forced to issue from the globe by another pipe fixed in the stopper of the orifice, and communicating with the pneumatic tub.

This process is exactly the inverse of that which I proposed in my work on *aërostats* \*, to fill a balloon of inflexible matter with hydrogen gas. It was founded on the same principle—the difference of the specific gravity of the two fluids. Here it had the advantage of leaving the vessel perfectly clean; an important condition, and which it is so difficult to obtain when air is expelled by mercury.

It was readily foreseen that the first portions of the oxygen gas would become mixed with the atmospheric air, and that it would be necessary to displace this mixture several times by new quantities of oxygen gas, that no more azotic gas might remain in it, or, at least, that the remaining quantity should be so small as to be incapable of having a sensible effect on the results of the experiment. We had even contrived means to determine it, by receiving under the pneumatic bell the

\* Description de l'aërostat de Dijon, &c.

last portions displaced, that we might subject them to a eudiometric proof.

With this view we employed 18 decagrammes (about six ounces) of the oxygenated muriat of pot-ash, which were put into a retort, at once to furnish, at one operation, the whole quantity of the gas necessary for this renewal.

Those who have not tried themselves this method of substituting one æriform fluid for another, might entertain some doubt respecting the purity of that employed in our experiment; but it will be easy for me to remove it. This was an article of so much importance that we could not neglect attempting to collect proofs of it.

We know with what success M. Humboldt applied to the improvement of eudiometry. The interest which he took in our experiment induced me to invite him to come and determine himself with those instruments, and by those processes, which were familiar to him, the purity of the oxygen gas in which the combustion was to take place. He readily accepted my invitation, as I had reason to expect, from his well known zeal for the progress of science; and this article of our report is the production of his pen. I had likewise the satisfaction of seeing him apply eudiometric instruments to the examination of the residuum of the gas after the combustion. The following is the result of the proofs to which he subjected that introduced into the apparatus to serve for the combustion:—

Nitrous gas, disengaged, by means of copper, from weak nitric acid, was tried with sulphat of iron and oxygenated muriatic acid, which shewed in it from 0.09 to 0.10 of azot. One hundred parts of oxygen gas received, towards the middle of the operation, as it issued from the globe, in which we had proposed to displace, in succession, common air by oxygen gas, were mixed with 300 parts of this nitrous gas: there was an æriform residuum of 0.66: making allowance for 0.27 or 0.30 of azot, pre-existing in the nitrous gas, we judged that, in 100 parts of gas tried, there were still 36 of azotic gas.

We then continued to introduce into the globe fresh oxygen gas. We collected towards the end a portion which was

subjected to the same proof, in the same proportions, with the same nitrous gas. The residuum this time was only 30 parts; and as the 300 of nitrous gas employed contained nine or ten parts for  $\frac{2}{3}$  of azot, we concluded that this oxygen gas was quite pure.—These are the words of M. Humboldt.

When this first condition was fulfilled, the question then was to place, in the centre of this globe, the diamond destined for the experiment. We had previously formed a small cup of the lower portion of a furnace-pipe, the tube of which, five centimetres in length, was fixed to an iron stalk, and this stalk was stuck into a cylinder of cork destined to be inserted in the neck of the balloon. This cork was dipped in mastic to shut its pores, and a small glass tube passed through it to establish a communication between the inside of the balloon and the mercurial tub. (See fig. 2. Plate II.)

We then placed the diamond on the cup, where we left it, having put it there by means of a ribbon, which we drew from under it in an instant, as soon as the balloon had been inverted, and its neck immersed in the mercury. The balloon in this position was made fast in a kind of collet, which rested on the edges of an iron mortar that served as a hydrargyro-pneumatic tub. We then exhausted, by suction, a portion of the oxygen gas sufficient to make the mercury rise to 12 centimetres above the orifice of the balloon.

The diamond was the same that had been already exposed to the action of the solar fire towards the end of the experiment of the preceding year, and which had lost only two deci-milligrammes, and consequently weighed 199.9 milligrammes (3.766 grains), or one carat wanting  $\frac{1}{32}$  \*.

On the 5th Fructidor last year, at one in the afternoon, we began to throw upon the diamond the focus of the large lens of the National Institute. The thermometer, exposed to the sun under a bell-glass, indicated 39.75: the mercury in the barometer stood at 75.89 centimetres, (28 inches 0.5 lines.) The volume of air, inclosed by means of the mercury in the tub, brought to the pressure of 757.7 millime-

\* It is well known that the carat of the jewellers is only 205.72 milligrammes.

tres (28 inches), and to the mean temperature of 12.5 degrees of the decimal thermometer, according to the experiments of Cit. Pncier, and the tables of Cit. Prony \*, was then found to be 11,470 cubic centimetres.

Having taken the necessary precautions to heat gradually the balloon, the point of the luminous cone being, almost in the centre, we were obliged to cover with a glass plate the wooden supporter, which was already on fire. The diamond first exhibited a black point at the angle immediately struck by the sun. We afterwards saw it entirely black, and as it were charred: we distinctly perceived, a moment after, brilliant points in a state of ebullition, as it were, on the black ground. The solar rays, having been for a moment intercepted, it appeared transparently red. The sun becoming obscured by a cloud, we saw it of a much purer white than it had been at the commencement of the operation.

The sun having emerged from the cloud, the surface of the diamond assumed the appearance of metallic splendour: it was then sensibly diminished, and there remained no more than a quarter, of a lengthened form, without angles or perceptible edges, but still very white, and of a beautiful transparency. We observed a slight fissure at the bottom of the pipe which supported it, but without any separation of the parts. I must not forget, that at the commencement of the combustion we thought we observed a purpurefcent cone arising from the support in the pencil of the solar rays; but this phenomenon was only an optical effect, which depended on the position of the observer.

The whole apparatus was left in the same state, only defended by an inverted box placed over it, until the 7th, when we again began, at one hour twenty minutes, to present the diamond to the focus. We soon observed the same phenomena as on the 5th, the black surface, the brilliant points in ebullition, which vanished and re-appeared according to the intensity of the focus: we saw also a brilliant metallic appearance, or rather leaden-colour. This is the expression which the assistants employed to characterise this phenomenon. At one hour forty minutes the diamond was entirely

\* Journal Polytechnique, Part II. p. 65.

consumed.

consumed. We at first suspected that there still remained a brilliant particle; but we soon judged that it was a vitrified point of the support, which was confirmed on inspecting the pipe when drawn from the globe. The question now was to collect the products of the combustion. No means seemed likely to be attended with more certainty than to introduce water of barytes into the apparatus, taking care to adhere, as nearly as possible, to the proportions indicated for the saturation of the carbonic acid, which we supposed must have been formed to prevent the uncertainty which the excess of this re-agent might occasion in the results. The whole apparatus being removed into the shade, we began on the 9th to draw out the pipe which served as a support, and observed on it two slight fissures occasioned by the contraction, and a spot of four or five millimetres in diameter, the centre of which had a vitreous appearance, and its edges a reddish hue. On examining it with a magnifying glass, we observed at the lowest point a space of two or three millimetres diameter, the surface of which was really vitrified, but of a tarnished and unequal colour. We distinguished a particle of white glass, pure and brilliant, formed into a globule some smaller portions of the same nature, and two small globules of a vitreous substance, which had a greenish red colour.

On one side we observed on the edges several other very small globules interspersed in a yellowish ground, and on the opposite a slight tinge of very bright red with very small reddish points. A particle of white earthy matter was at first taken for a fragment detached from the edges of the pipe, but it was found friable, and ascertained, by the stain it left on gold, to be oxyd of mercury. This examination being finished, we introduced into the globe five measures of saturated water of barytes, each of 46.5 cubic centimetres. The liquor immediately assumed a milky appearance, and there was a diminution in the volume of gas, which, calculated by means of the attached paper scale, amounted nearly to 300 cubic centimetres. Thus we might stop here and consider the experiment as terminated, and by making some allowance for the errors unavoidable in such manipulations, and make the result tally with the amount, determined before by the noble experiment of Lavoisier and La Place, of the re-

spective quantities of carbon and oxygen which form the carbonic acid. Mr. Tennant seems to have done the same thing lately after the combustion of the diamond by nitre. But we should only have confirmed what was before known, or supposed to be known. Our object was not only to observe, with more attention, what took place during the act of combustion, but to ascertain, as accurately as possible, the nature and quantity of the product, and the reader will find that the labour undertaken on this subject has not been fruitless.

The liquor was agitated in the globe to mix the white matter which had been deposited. We drew out  $4\frac{3}{4}$  measures of the five we had introduced by making use of the same inverted bottle filled with mercury, and which we raised on the inside by means of an iron stalk composed of several pieces, which could be adjusted by screws. (See fig. 6. Plate II.)

We introduced into the balloon three new measures, each containing the same quantity of distilled water, which was shaken in the inside to detach and collect what adhered to the sides. These united liquors, being immediately filtered in an open filtre, left 192 centigrammes ( $36\cdot142$  grains) of carbonat of barytes dried in the heat of boiling water.

It may be readily judged what was our astonishment when proceeding to examine the liquor, instead of finding in it a slight excess of uncombined barytes, we observed that it changed neither the colour of turmeric nor that of logwood, and that, on the contrary, it acted on an infusion of turnsole as water charged with the carbonic acid. The presence of this acid unequivocally manifested itself, when we poured upon it a few drops more of barytes water, which immediately rendered it turbid. It was necessary to add even  $4\cdot65$  centimetres of this water to saturate and precipitate the remaining acid gas. Being informed by this phenomenon that the production of the gas had been more considerable than we expected, and that some of it still remained mixed in the æriform fluid in the balloon, we took every measure necessary to determine the quantity. This we were luckily enabled to do by the divisions which had been marked on the scales, the orifice of the globe having never been yet taken out of the mercury.

When the barytes water was taken out, the apparent volume

lume was found to be exactly 122 decilitres, the internal column of the mercury above the level of the tub was 47 millimetres; the barometer being at 759.96 millimetres, (Fruëtidor 19, year VI.) the centegrade thermometer at 21.25, the real volume, at a mean pressure and temperature, was 112.426 decilitres, or 11242.66 cubic centimetres.

I still invited M. Humboldt to co-operate with us in examining the nature of this residuum of gas. It was transferred in his presence into a pneumatic tub prepared on purpose with distilled water, and received into four large flasks. The trial was made by the same instruments. and with the same nitrous gas, which had served for the oxygen gas before the combustion, and consequently containing from 0.09 to 0.10 of azotic gas. The trials made on portions extracted from different flasks varied from 31 to 34 in the quantity of the residuum of gas, in a mixture of 100 parts of gas examined with 300 parts of nitrous gas.

I shall not even take the mean term, I shall stop at the weakest, which indicates four hundredth parts of carbonic acid gas, which, I think, I can assert to be rather below than above the truth; since a portion of this same gas, brought into contact with ammonia, under a receiver, experienced a diminution of 4.5 *per cent.*

Let us now estimate the carbonic acid gas which entered into the composition of the 192 centigrammes of carbonat of barytes. According to Pelletier, whose accuracy is well known in researches of this kind, 100 of this earthy salt contain 22 of acid gas \*, which gives 42.24 for 192; and as the cubic centimetre of gas weighs 1.847 milligrammes, it follows that the 42.24 centigrammes represent 228.621 cubic centimetres. If we now add, on the one hand, the 449 cubic centimetres, found in the residuum of the gas after combustion, and which, as we saw, formed the four hundredth parts; and deduct, on the other, the same quantity from the aeriform fluid in which the combustion was effected, it results, that in 11470 cubic centimetres of oxygen gas contained in the balloon, there remained, after the combustion, only 10793; that 677 were consumed; that these 677 cubic centimetres of

\* *Annales de Chimie*, Vol. XXI. p. 135.

oxygen gas, in the ratio of 1.3577 milligrammes each, produced, with the 199.9 milligrammes of the diamond, 1117.96 milligrammes of carbonic acid.

In the last place, that, instead of the proportions 0.28 of combustible substance, and 0.72 of acidifying principle, observed in the combustion of carbon, the proportion was, for the combustion of the diamond, - 17.88 of carbon.  
82.12 of oxygen.

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100.00

Though it was not possible for me to doubt facts deduced from calculation, I at first hesitated to admit differences so considerable in the manner in which the same combustible united itself to oxygen in the quantities it could take up, and the products of its combustion; in a word, a carbonaceous combustible more abundant in real combustible matter than charcoal itself, and which at the same time differed so much from it in the degree of temperature necessary to determine the action of its affinity. But I soon began to reflect, 1st, That this would not be the only instance of the first degree of the oxydation of an acidifiable base having been operated with great difficulty, while the acidification was afterwards completed with the utmost facility: 2d, That several substances of the same kind presented to us also these two characters; a greater abundance in real carbon, and greater resistance to inflammation; so that they naturally placed themselves in an intermediary rank between the diamond and charcoal. These two considerations, still strengthened by the similarity of the phenomena observed during the course of our two experiments in the passage of the diamond to the state of carbonic acid, appeared to me to throw a ray of light on this subject hitherto so obscure.

In regard to the first consideration it will be sufficient for me to call to mind with what difficulty the commencement of a composition of azot and oxygen is formed by the direct way, and the high degree of temperature which it requires, while nitrous gas cannot be in contact with oxygen without passing immediately to the acid state. Charcoal will then be to the carbonic acid what nitrous gas is to the nitric, and the



the diamond will be to charcoal what azot is to nitrous gas. There will, therefore, be no longer occasion of wonder that more oxygen is necessary to that substance, which as yet has none of it, than to that which has already been united with the quantity necessary for arriving at the first point of saturation.

The second consideration rests on facts no less conclusive. Plumbago is a carbonaceous combustible, which does not burn but at a very high temperature, or in nitre in fusion; which produces by its combustion carbonic acid; which, as well as the diamond, is more abundant in combustible matter than carbon itself. We are indebted to the illustrious Scheele for the first observation of this fact. One part of carbon alcalises only five parts of nitre; one part of plumbago can alcalise ten. The operation performed in a retort on 80 centigrammes of plumbago, gave him 357 cubic centimetres of carbonic acid gas\*. This agreement will not be contested by those who, having been witnesses of our experiment, so unanimously declared, that the surface of the diamond assumed instantaneously a leaden colour.

This mineral is not the only body which presents these striking characters of a substance almost incombustible, and yet very abundant, in combustible matter. I described, sixteen years ago, in the Memoirs of the Academy of Dijon, a fossil found in a mass in the coal-pits of the Rive-de-Gier, which was sent to me under the name of *incombustible coal*, and which I then considered as real coal which had passed to the state of plumbago. I characterised it in that manner.

Our brother Dolomieu has described a fossil of the same kind, which he calls *carbure of alumine*, which is the *anthracolite* of Werner. I had already suspected that it was neither the presence of four or five centiemes of alumine, nor that of a still smaller quantity of iron, that rendered it incombustible, but the little advanced state of the oxydation of the carbon. I subjected it to two experiments, by which this was fully confirmed.

The object of the first was to determine if the alumine present was in a state of combination sufficiently intimate to

\* Mem. de Scheele, French edit. Vol. II. p. 27 and 29.

resist the action of pot-ash by the humid way : 100 parts, put in digestion in that solvent, left in it 4.6 of alumine.

The second was, to ascertain whether this combustible, which possessed so little inflammability, had also the power to alcalise more nitre than carbon, consequently to take up more oxygen. Three successive trials gave for a mean result the alcalisation of 7.87 parts of nitre by one part of that mineral; and the same coally matter, digested for four or five days in oxygenated muriatic acid, burnt completely with 6.5 of nitre.

M. Klaproth, the celebrated chemist of Berlin, had before submitted to trials of the same kind a fossil described by M. Widenmann under the name of *incombustible coal*, and found that 100 parts left, after combustion at a very strong heat, only seven of a cineritious residuum; that treated in a crucible with eight parts of nitre, and the mass dissolved in water, acids occasioned no precipitate. Kirwan, in his experiments on coal, remarks, that that which he calls *Kilkenny coal*, and which has a metallic brilliancy, which does not burn but when carried to incandescence, and which then consumes slowly without emitting flame, can decompose 9.6 of nitre.

After this I do not see how there can remain any doubt that these supposed incombustible substances are real oxydes of carbon, which, like coal or charcoal, have the property of conducting the electric fluid; of cementing iron; of taking the oxygen from some acidifiable bases; but which are not at that degree of oxydation necessary for exercising this separating affinity at a weak temperature.

I must not omit this opportunity of making some application of this principle, which may become useful to the arts. It has not yet been sufficiently explained, why some animal and vegetable matters produce carbon so difficult to be incinerated; why charred pit-coal, known under the name of coke, or cinders, and which has been half burnt in the preparation, is, however, so powerful a combustible; why peat, or turf, the weakest of combustibles, acquires, by being properly charred, the property of welding large pieces of iron better than charcoal; and why, in the last place, charcoal, when exposed to a very strong heat in vessels impenetrable

to air, becomes there, in a certain degree, incombustible, as is proved in the experiment made by Mr. Tennant, which I have mentioned in the article Air in the *Dictionnaire de Chemie Encyclopedique*, Vol. I. p. 714.

The answer to all these questions may be found in the theory I have laid down: they are charcoals in the first degree of oxydation. Thus some of them have not yet acquired that which constitutes charcoal properly so called; others, after possessing all the qualities of vegetable and mineral carbon, have returned to the first degree by a real (*debrullement*) unburning of the remaining carbon; so that by losing their inflammability they become capable of fixing a greater quantity of oxygen, and consequently of setting at liberty a greater quantity of caloric when they find themselves at a temperature sufficiently high to determine and complete their acidification.

Some practical consequences will doubtless hence be deduced in regard to processes for the reduction of metals; for the cementation of steel, which it is probable takes up only oxyd of carbon, since it is separated from it in that state; for the incineration of the carbonaceous residuums of our analyses; for the carbonisation of wood, pit-coal, and turf: in a word, we may perhaps thence conclude the possibility of rendering useful those masses of pit-coal, said to be incombustible, found at Rive-de-Gier, by mixing it with more inflammable matters, to maintain the temperature which determines its combustion. Its position, texture, and all its exterior characters announce, as already said, that it consists of beds of coal changed by a subterranean fire; and this is confirmed by tradition, which preserved to that mountain, for three centuries, the name of the *Mountain of Fire*. (*Montagne de Feu*.) We can now pronounce that it is coke too far advanced, but so much the more susceptible of producing a great heat under favourable circumstances.

#### RECAPITULATION.

I shall here enumerate the consequences, or rather the facts, which result from the phenomena observed in the two

combustions of the diamond by the solar fire, and the experiments which followed:—

1. It is not only by the colour, weight, hardness, transparency, and other sensible characters, that the diamond *differs* from charcoal, as seems hitherto to have been believed;

2. Nor is it by the state alone of the aggregation of the matter that constitutes diamond:

3. Neither is it on account of the 200th part of the cineritious residuum left by carbon, or the small quantity of hydrogen which it contains.

4. It is more essentially by its chemical properties that it differs.

5. The diamond is the pure combustible substance of this genus.

6. The product of its combustion, or of its combination with oxygen to saturation, is carbonic acid without residue.

7. Carbon burns at a temperature estimated at  $188^{\circ}$  of the centigrade thermometer; the diamond does not inflame but at about 30 pyrometric degrees, which, according to Wedgwood's scale, makes a difference of 188 to 2765.

8. Charcoal, when kindled, maintains of itself, in oxygen gas, the temperature necessary for its combustion. The combustion of the diamond stops when you cease to maintain it by a furnace-heat, or the union of the solar rays.

9. The diamond, for its complete combustion, requires a much greater quantity of oxygen than charcoal does, and produces also more carbonic acid. One part of charcoal absorbs in this operation 2.527 of oxygen, and produces 3.575 of carbonic acid. One of diamond absorbs a little more than four of oxygen, and really produces five of carbonic acid.

10. There are substances which are in a state of intermediary composition between the diamond and charcoal. These are plumbago, or native carburet of iron; incombustible fossil coal; the carburet of alumine of Dolomieu; the anthracolite of Werner; the black matter united to iron in the state of cast iron and steel; carbonaceous residuums difficult to be incinerated; and carbon itself unburnt, (*d'brulé*), by the action of a strong heat without the contact of air.

11. These substances mixed, or weakly combined with three or four hundredths of their weight of iron, or alumine, give by their combustion carbonic acid, like charcoal and the diamond.

They approach to carbon by their colour, their lightness, their opacity; by their serving, like it, to decompose water, to cement iron, to deoxydate metals, to deoxygenate sulphur, phosphorus, and arsenic; and by conducting, like it, the electric fluid. They approach the diamond by containing more combustible matter than charcoal; by absorbing also more oxygen, and producing more carbonic acid; by decomposing more nitrous acid; by burning only at a much higher temperature, even in nitre in fusion; and by their combustion being stopped when this temperature is lowered. They seem to differ from each other by the property of producing with zinc galvanic irritation, as well as silver does: which can be effected neither by the diamond nor charcoal.

12. Thus the diamond is pure carbon, the pure acidifiable base of the carbonic acid. Its combustion is effected in three periods, which require three different temperatures. At the first, which is the highest, the diamond assumes a black leaden colour. It is an oxydation in the first degree, the state of plumbago and anthracolite. At the second temperature, which may be estimated at 18 or 20 pyrometric degrees, there is a second slow and successive combination of oxygen. It is a progress of oxydation which constitutes the habitual state of charcoal, or rather that in which it is found after the action of a strong heat in close vessels has disengaged a part of its oxygen.

Thus plumbago is an oxyd of the first degree, charcoal an oxyd in the second, and the carbonic acid the product of the complete oxygenation of the carbon.

Supposing, then, that we operate with sufficient precision to take away from the surface of the diamond the black matter in proportion as it is formed, by suddenly withdrawing from it each time the action of the solar fire, we should doubtless be able to convert it into charcoal, or at least plumbago, if the too rapid passage of the last degree of oxydation to oxygenation did not prevent us from surprizing it in that state.

13. In the last place, several consequences, of importance to chemistry and the arts, arise from these principles.

After this conclusion it will be asked, no doubt, how it happens that the simple matter, pure carbon, the diamond, is so rare while its compounds in different states are so abundantly diffused? To put an end to the astonishment of those who might entertain any mistrust, I shall observe, that aluminous earth is also one of the most common matters, and that adamantine spar, as rare as the diamond, is however only alumine; that iron every where exists, under all forms, except in the state of purity; the existence of native iron is still doubtful. The wonder exists only in the opposition of facts to our opinions, and will disappear in proportion as we discover, and appropriate to ourselves, the means employed by nature in producing the same effects.

Those who have never turned their attention to the physical sciences, to estimate at least their influence on public felicity, are disposed to treat as vain curiosity labours which are not immediately directed towards a near object of new enjoyment. What would have been their astonishment had they been told, that researches on the nature of the diamond would one day produce truths which might give rise to happy changes in the practice of the most familiar arts; in the preparation, and in the employment of the coarsest combustibles! Such, however, are the consequences that may arise from the best known properties of the essential principle carbon in its different states.

XI. *A Letter to the Editor, containing some Objections to the Mitchillian Theory of Pestilential Fluids.*

SIR,

I OBSERVE, in the 15th Number of your useful Magazine, a communication from Dr. Mitchill, of New-York, shewing the utility of constructing the houses, and paving the streets of cities, with *calcareous* in preference to *siliceous* and *argillaceous* materials. This communication is contained in a letter from Mr. Da Costa to the Doctor, in which

it

It is observed, that although the city of Lisbon is one of the filthiest in Europe, and the most infested with putrid effluvia, yet that it nevertheless is remarkable for its salubrity. This is attributed by Mr. Da Costa to the calcareous materials, of which the buildings and pavements are constructed, absorbing and neutralising the septic fluids, the cause, according to Dr. Mitchill's theory, of all contagion.

In reply to this I would observe, that from the author's own account it is evident that the calcareous earth in the city of Lisbon does *not* destroy the putrid exhalations in any sensible degree, and therefore cannot prevent any disorders to which they may give rise. The houses, he observes, are very offensive, from the privies, and from inattention to cleanliness. The mouths of the sewers go to the wharves, and are bare at low water. The fluid which comes from these contains so many infectious matters that its *strong putrid smell can scarcely be endured!* It is plain, therefore, that the putrid exhalations are not destroyed, or, at least, that they are so quickly generated as to annoy the olfactory sense in an intolerable degree. The bodies of the inhabitants are constantly immersed in putrid exhalations, yet contagion is not produced. The inference is, that putrid exhalations are not its immediate cause. Contagion arises where no putrefaction is going on, at least in any degree perceptible to the senses; as when a number of persons are crowded together in jails, and even in the houses of the poor in the winter season, without sufficient ventilation. On the other hand, putrefaction may be present, in a very high degree, without giving birth to contagion, as may be observed in slaughter-houses, cat-gut manufactories, &c.

But, is it proved sufficiently that calcareous matters do absorb and neutralise septic exhalations? Mr. Da Costa remarks, that he has observed, two or three times, in Lisbon, dead animals upon the ruins of houses, and of course surrounded by calcareous earth, in a state of desiccation; and, at the same time, two or three fathoms distant, another animal dead too, and lying upon another kind of soil, in a state of complete putrefaction. I do not presume to question the truth of this, since it was a matter obvious to the senses: but

I deny

I deny the generality of the fact equally from observation. I suspended a piece of flesh, in a proper vessel, over a layer of chalk, and at the same time laid chalk, in small pieces, on gauze, a few inches above the flesh: but I did not observe that putrefaction was more backward than in other circumstances, or that putrid exhalations were less extensively diffused around. That quicklime destroys putrefaction, is no proof of the opinion here advanced, since this can be accounted for on other principles.

If fevers abound on the opposite side of the Tagus, whilst the city of Lisbon is free from them, there are probably other causes to which this may be ascribed, such as a marshy soil, &c. Whether any thing of this sort exists, the account here given by Mr. Da Costa does not enable us to judge.

It is the tendency of Dr. Mitchill's theory to overturn settled opinions, and to cause us to abandon practices, which have been generally employed, and much relied on, for the destruction of contagious matter. It is of great importance, therefore, that it should be well founded, lest we be induced to lay aside means that are efficacious on grounds not sufficiently established. Whatever becomes of Dr. Mitchill's hypothesis, it appears to me to receive no support from the facts and arguments of the paper in question, although it is considered by him as affording a strong confirmation of its truth.

Wishing every success to your valuable publication, I remain, Sir, yours, &c.

H. CLUTTERBUCK,

Walbrook, Oct. 21, 1799.

## XII. *Description of Mr. HOWARD's Improved Air-Furnace.*

THE difficulty of obtaining a degree of heat sufficiently intense for many operations in chemistry, has been felt and lamented by every one engaged in such pursuits; and, notwithstanding the researches and numberless experiments made professedly for that purpose by the greatest men, the best construction of an air-furnace is still a problem; indeed,



deed, the rules laid down by those who have written on this subject, differ so widely from each other, that we must suppose a great number of circumstances, hitherto overlooked, enter into the construction of a good furnace. Having observed effects produced in that of Mr. Edward Howard, of Doughty-street, which we have seldom, if ever, seen equalled in any other furnace, we thought we could not better oblige our chemical readers than by giving a description of it. The difference in the present composition of Wedgewood's pyrometer pieces, from that used by him when he first invented his instrument, makes it impracticable (unless the scale was also altered) to measure correctly the degree of heat obtained; it must however be at least  $= 160^{\circ}$ , and is sufficient to run down the best Hessian crucibles. The most striking deviation from the common construction is in the lower part of the chimney, or flue, being smaller than the upper, and the greater depth of the ash-pit. Mr. Howard thinks that something depends on the direction of the horizontal funnel, which in this case opens to the north. If any of our readers should construct a furnace on this plan, we should be glad to be informed of the coincidence of effect\*.

Fig. 1. (Plate IV.) A, the cavity or body of the furnace, 9 inches square and 1 foot 7 inches deep to the bars. B, the ash-pit, 1 foot 2 inches, by 9 inches broad, and 2 feet 8 inches deep below the bars: this ash-pit opens to the external air by an horizontal funnel C, 6 feet long, passing under the floor of the laboratory: this not only furnishes a supply of denser air, but prevents the unpleasant effects of the cold draught on the legs and feet of the operator, which happens when the ash-pit opens into the laboratory. The external opening of this funnel is about 2 feet square, which gradually contracts to the ash-pit in the manner shewn in the plate. D, the aperture of the horizontal flue, 7 inches wide by  $2\frac{1}{2}$  deep, contracting in width to  $4\frac{1}{2}$  inches, where it enters the vertical flue or chimney, which is  $4\frac{1}{2}$  inches square, of which width it continues to the height of 7 feet, and is then enlarged to 5 inches square; which dimensions it preserves to

\* Dr. Pearson's furnace, which is a very powerful one, agrees in some parts of its construction with Mr. Howard's.

the top of the brick-work, being  $16\frac{1}{2}$  feet from E. The chimney finishes by an iron pipe about 3 feet long.

Fig. 2, F, shews the plan of the body of the furnace 9 inches thick, the top course of bricks being bound together in the usual manner by a strong iron hoop G. H is an horizontal section of the chimney and flues\*. When only a moderate degree of heat is wanted, the horizontal draught-hole C can be closed by a sliding door or register, and an opening made into the ash-pit, directly under the bars, by removing the stone stopper I. (Fig. 1 and 2.) This also is useful for occasionally clearing the bars from scoria, or entirely removing them, in order suddenly to put out the fire. The whole of the furnace and chimney is built of Windfor bricks.

### XIII. *An Account of Mr. BROWN's Travels through Egypt and Syria, &c.*

[Concluded from Page 76.]

MR. BROWN retired a second time to Cobbé, with little hopes of ever leaving the country. Of the property, which the king's agents had purchased on his arrival, no part of the price had yet been paid. He had been insulted with the mockery of justice, yet obliged to thank his oppressors for the compensation which their corruption and malignity alone had rendered incomplete.

He had not omitted to renew to the Melek Mufa the request which had been previously made to Misellim and Ibrahim. He explained to him, in the least exceptionable manner, his intention of coming to Cobbé; completely did away all the suspicions which his enemies had at first excited; and concluded with desiring permission to go to Sennaar, or to accompany the first *selatea* (an armed expedition for the purpose of acquiring slaves,) to the south or south-west; or to have a safe conduct, and one of the Sultan's slaves, to accompany him to Bergoo, (the first Mahometan kingdom on the west.) By the first route he hoped to have reached Abyssinia, or, if that had been impracticable, to have gone through Nubia to Egypt, or by Suakim to the Red Sea, and thence

\* The chimney is detached about an inch from the wall behind all the way to the top.

to Mocha or Jidda. By the second he was almost certain of settling some important points relative to the White River, possibly of tracing it to its source. And by the third, either of passing directly west, and tracing the course of the Niger, or of penetrating through Bornou and Fezzan to Tripoli.

To the first proposal he answered, in a manner which gave Mr. Brown reason to doubt his sincerity, that the road to Sennaar was unpassable, the Sultan being then master of only one half of Kordofan: that the natives of all that part of it which remained unsubdued were his implacable foes, and would infallibly destroy any person who came from Dar-Fûr: that he thought, however, if Mr. Brown waited another year, that route might possibly be more secure; and in case it should, he would use all his efforts to obtain the Sultan's permission for his departure.—Of the *Selatea* he said, that our traveller would only encounter certain death by attempting it, as, between the jealousy of those who accompanied him, and the actual hostility of the country, there would be no hope of escaping. Mr. Brown hinted, that the Sultan might give him a few attendants, whom he was very ready to pay, and an order to enable him to pass unmolested as his physician in search of herbs. He replied, that he would propose such a measure, but he did not expect it would receive the Sultan's approbation.—To the third proposal he answered, that he had no hope of Mr. Brown's succeeding, and concluded with strongly recommending to him to seize the first opportunity of returning to Egypt; but he assured him, if he could accomplish any of the measures he so much wished, he would not fail to inform him and to afford him the necessary aid. Such was the state of affairs when Mr. Brown returned to Cobbé, dejected, and with little expectation of realising even his least sanguine hopes. Not more than six weeks after this conversation had taken place, he was sent for to attend the Melek, who was confined by an old disorder in his lungs. He found him yet sensible; but his eyes were fixed, and extremities incapable of motion. In five hours after, he expired. Thus were blasted our traveller's returning hopes of success; for no mediator now remained between him and the monarch, and no longer was there

near the court a man even of seeming liberality and good sense, to whom his projects might be safely opened.

During the summer of 1794, five men who had exercised considerable authority in some of the provinces, were brought to El Fasher as prisoners; it was said that they had been detected in a treasonable correspondence with the hostile leader, Hashem, in Kordofân. They did not undergo any form of trial; but as the Sultan chose to give credit to the depositions made against them, his command was issued for their execution. Three of them were very young men, the youngest not appearing to be more than seventeen years of age. A little after noon they were brought, chained and fettered, into the market-place, before one of the entrances of the palace, escorted by a few of the royal slaves armed with spears. Several of the Meleks, by the monarch's express order, were present, to witness, as he termed it, what they might expect to suffer if they failed in their fidelity. The executioner allowed them time only to utter a short prayer, when he plunged his knife into the neck of the oldest, exactly in the same manner as they kill a sheep. The operation, too, is marked by the same term, *dbebbab*. He fell, and struggled for some time. The rest suffered in their turn. The three last were much agitated, and the youngest wept. The two first had borne their fate with becoming firmness. The crowd that had assembled had scarcely satiated itself with the spectacle of their convulsive motions while prostrate in the dust, when the slaves of the executioner coolly brought a small block of wood and began mangling their feet with an axe. Having cut off their feet, they carried away the fetters which had been worn by the criminals, though of little value, and left the bodies where they were. Private humanity, and not public order, afforded them sepulture.

Near the end of the year 1795 a body of troops was mustered and reviewed, intended to replace those who had died of the small-pox in Kordofân, which, it was said, amounted to more than half the army. The spoils which had been taken from Hashem were also ostentatiously displayed on this occasion. They consisted of eighty slaves, male and female, but the greater proportion of the latter, many of whom were exceedingly

exceedingly beautiful, nor the less interesting, that, though the change in their situation could not be very important, their countenances were marked with despondency. To these succeeded five hundred oxen and two hundred large camels; the whole procession was closed by eighty horses, and many articles of less value borne by slaves. Shouts rent the air of, Long live El Sultan Abd-el-rachmân el rashîd! May God render him always victorious!

A short time after, Mr. Brown caused a petition to be drawn up, which was presented by Ali-el-Chatib to the Sultan, in which he stated his sufferings, requested payment of what yet remained due to him, and permission to proceed on his journey to Kordofân. Though the person who presented it was a man of considerable weight, no answer was given. He therefore followed it up by a visit in person, which he had resolved should be his last. His arrival was no sooner known than he was directed to attend some sick person, as he had several times done before. This he positively refused, and it was many days before he could be admitted at court. On the 11th of December 1795, however, he accompanied the Chatib to the monarch's presence, and briefly stated what he came to request; which the former seconded, though not with the zeal Mr. Brown wished. To his demand of permission to travel, no answer was returned. But the generous and hospitable monarch, who had received from him the value of 750 piastres in goods, though his claim was well supported, gave him only twenty meagre oxen, in value about 120 piastres! The state of his purse would not permit him to refuse even this mean supply; and he bade adieu to El Fasher, as he hoped, for ever.

Having applied the value of the oxen to preparations for his journey to Egypt, the report of the caravan's departure growing daily stronger, he lost no time in joining the Chabîr, who was then encamped at Le Haimer, (3d March 1796,) a small village about three days journey north of Cobbé, where there was a tolerable supply of water, but no other requisite for living.

Mr. Brown arrived at Le Haimer about a month before Ramadan; and it was not till the sixth day of El Hedge, the

second month after that fast, that they actually commenced their journey to Egypt. In the mean time, having pitched a tent under a great tree, where they were sheltered from the rays of the sun, he fed on polenta (*as-eid*) and water with the camel-drivers. He had collected eight camels for the journey, but the best of them was stolen while grazing;—another died; and to supply his place he was obliged to seek one on credit, for his whole exchangeable property at that time amounted only to eight piasres.

The journey, once commenced, was continued with little remarkable except violent heat. They returned by the only caravan route, *Bir el Malab, Legbea, Selime, Sbeb, and El-awab*. Their provisions were indifferent, and in small quantity. The camel-drivers regaled themselves with the flesh of these animals when they chanced to be disabled on the road. When they arrived at *Beiris*, they were met by a Castles, who welcomed the Jelabs with an exhibition of fire-works. On this occasion it is customary to treat the chief merchants with coffee, and to present to each a *ben-sh* of coarse cloth, worth about a guinea; but he expects in return a slave from each, worth about ten guineas. When Mr. Brown arrived at Affiût, he had been four months without eating animal food; and the hard living, heat, and fatigue, had brought on a diarrhoea, by which he was much weakened; but before he left Affiût, where he passed twenty days, it was much abated.

After some stay in Egypt Mr. Brown embarked for Syria on the 19th of January 1797; and having visited Jerusalem, Damascus, Aleppo, and various other places, proceeded to Constantinople; from which he returned to England by Vienna, Dresden, Leipzig, and Hamburgh. He arrived in London on the 16th of September 1798, after an absence of nearly seven years.

**XIV. Tenth Communication from Dr. THORNTON, Physician to the General Dispensary, &c. &c. &c. relative to Pneumatic Medicine.**

ONE of the most dreadful of human afflictions is that where water oppresses the brain; and there are no hopes of any

any medicine getting to that part, except what first can enter the blood. Mercury has been successfully employed; but it produces so great a derangement of the general health, that a substitute is certainly advisable. Can the absorbents of the brain be equally excited by vital air?

Lydia Johnson, æt. 13, living at No. 5, Husband-street, after a putrid fever became perfectly blind, and had frequent fits, evidently arising from oppression of the brain. Various means had been employed without the least advantage. I referred her to Messrs. Wathen and Phipps, that they might see the case. Under the inhalation of vital air, with the aid of bark and steel, the fits soon gave way; and afterward, finding the air produced no mischief, I increased the quantity one day to twelve quarts, mixed with three times that quantity of atmospheric, and almost immediately after the inhalation she had a dawning of sight, and going home she did nothing but cry out, *Mama, mama!* I see now every thing. The cure has remained permanent above a year, and I saw her the other day in perfect health and spirits.

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## INTELLIGENCE,

AND

## MISCELLANEOUS ARTICLES.

### ROYAL SOCIETY OF LONDON.

**I**N the course of the present month (November) the Second Part of the Transactions for the year 1799 was delivered to the Members. The contents are:—An account of the dissection of an hermaphrodite dog: to which are prefixed, some observations on hermaphrodites in general. By Everard Home, Esq. F. R. S.—An enquiry concerning the weight ascribed to heat. By Benjamin Count Rumford, F. R. S. M. R. I. A. &c. —An account of some experiments on the fecundation of vegetables: in a letter from Thomas Andrew Knight, Esq. to the

the

the Right Hon. Sir Joseph Banks, K. B. P. R. S.—Observations on the different species of Asiatic elephants, and their mode of dentition. By John Corse, Esq. Communicated by the Right Hon. Sir Joseph Banks, Bart. K. B. P. R. S.—Some observations on the structure of the teeth of graminivorous quadrupeds, particularly those of the elephant and *sus Æthiopicus*. By Everard Home, Esq. F. R. S.—Experiments to determine the quantity of tanning principle and gallic acid contained in the bark of various trees. By George Biggin, Esq. Communicated by the Right Hon. Sir Joseph Banks, K. B. P. R. S.—Essay on the resolution of Algebraic equations; attempting to distinguish particularly the real principle of every method, and the true causes of the limitations to which it is subject. By Griffin Wilson, Esq. Communicated by Edward Whitaker Gray, M. D. Sec. R. S.—On different sorts of lime used in agriculture. By Smithson Tennant, Esq. F. R. S.—Experiments and observations on shell and bone. By Charles Hatchett, Esq. F. R. S.—A catalogue of Oriental manuscripts, presented to the Royal Society by Sir William and Lady Jones. By Charles Wilkins, Esq. F. R. S.—Presents received from November 1798 to June 1799.

On the 7th instant the Society met for the first time since the long vacation, when the Croonian Lecture on muscular motion, by Everard Home, Esq. was read. The subject was on the construction of the membrana typani. At the same sitting a picture, by Mr. Brown, of the late Mr. Smeaton, civil engineer, was presented to the Society by Alexander Aubert, Esq.\*

Nov. 14. The conclusion of the Croonian Lecture was read, and a letter addressed to Count Bruhl on the orbit of the comet seen in August.

Nov. 21. A paper by Dr. Herschel on the power of penetrating into space by means of telescopes. This, the Doctor remarks, is connected with something different from their magnifying power. The extreme sensibility of the optic

\* The meeting-room is hung round with portraits of deceased members; that of Sir Isaac Newton being placed over the president's chair.



nerve is such as to require twenty minutes rest, on coming from the light, before (when his head is covered with a black hood) he is able to discern small telescopic stars: an equal time is necessary after a star of the second or third magnitude passes the field of the telescope.—The paper was not gone through at this sitting.

## METEORS.

Fiery Meteors have been more than usually common of late.

On Saturday night, the 2d of November, at half past ten o'clock, a meteor, or ball of fire, passed through the air immediately over the town of Pocklington, accompanied with a most beautiful train of fire resembling the tail of a rocket, and appearing to be about 15 yards in length. The direction was from the north-east to the south or south-west, and continued visible for about half a minute.

On Tuesday morning the 12th of November, about six o'clock, a meteor, or ball of fire, accompanied with a beautiful train, was observed in different parts of Staffordshire. It was preceded by several flashes of vivid lightning.

The same, or a similar meteor, was seen at the same time in Yorkshire. The following is a description of it, as seen at Hull and the neighbourhood:—On the morning of the 12th of November, between the hours of five and six, the heavens exhibited an awfully grand appearance. The setting moon became partially obscured by dark cloudy spots or streaks: in opposition to her was seen a lunar rainbow of the most beautifully varied colours; after which, the middle region of the air was illuminated by meteors crossing each other in different directions, and leaving behind them long sparkling trains, which were visible for two or three minutes after these luminous bodies had disappeared; one of these meteors, more brilliant than the rest, illuminated the whole firmament, and, by its apparent approximation to the earth, created some alarm. The thermometer was that morning at 50 degrees. The air, which the preceding night was cold and frosty, became remarkably close and warm, and produced on the walls

walls and furniture in houses an unusual dampness and humidity.

#### CONVERSION OF IRON INTO STEEL.

In our last we noticed that Mr. Mushet, in varying the experiments suggested by C. Clouet's process for the conversion of iron into steel by cementation with carbonate of lime, had found that when lime, *previously deprived of its carbonic acid*, was used with the iron, the result was, notwithstanding, cast steel; and that, therefore, he found himself obliged to reject the idea of the carbon necessary to the conversion of iron into steel, having been furnished by the decomposition of carbonic acid.

We also in our last Number laid before our readers C. Guyton's report of the conversion of iron into cast steel by means of the diamond, which is generally held to be pure carbon, and which, having disappeared in the process, was therefore believed to have entered into chemical union with the iron by the affinity ~~existed~~ between them by means of an high temperature.

From the tenor of the letter from Mr. Mushet, which furnished us with matter for the notice first above referred to, we were led to suspect that, however probable it might be that in the case last mentioned the carbon, which went to steelify the iron, was furnished by the diamond; yet, as Mr. Mushet's experiments went to shew that carbon could find its way from the ignited gas of the furnace to the iron, through materials which, on first view, one would hardly think pervious to such a principle, it was certainly possible that the French chemists might be mistaken, and that the carbon, which converted the iron, might not after all have been furnished by the diamond.

Since, then, we corresponded with Mr. Mushet on the subject, and proposed that the experiment made at the Polytechnic School should be repeated, only *keeping out the diamond*. As the idea arose from, and might indeed be said to be embraced in, the facts presented by Mr. Mushet; and as the process coincided so nearly with others, which he had communicated as executed and in train, we knew no person

so likely to do it complete justice, or so well qualified by great experience to guard against error. In this we have not been deceived. Mr. Musket, whose zeal in every thing that concerns the improvement of the British iron manufactory entitles him to the gratitude of his country, has proved, by several conclusive experiments, that the French chemists would have had steel from their experiment even if no diamond had been employed in it. We shall not detain our readers longer, but content ourselves with laying before them

*An Account of the Experiments made by Mr. Musket with a view to prove whether the Experiment made at the Polytechnic School respecting the Conversion of Iron into Steel by means of the Diamond is conclusive.*

I. "I introduced into a crucible some pieces of soft malleable iron weighing 1250 grains. A larger crucible was next taken, half filled with sand pounded from the fire-stone of which the blast-furnace hearths at Clyde are made. The small crucible containing the iron was inverted upon the surface of the sand in the second crucible, and forced so far into it as to bring the iron and sand nearly into contact. The space betwixt the exterior surface of the small crucible, and the interior surface of the large one, was completely filled with the same sand; the bottom of the inverted one also received half an inch of sand. This quite filled the outer crucible, and made it have the appearance of being entirely filled with sand: a well fitted fire-clay cover sealed the whole. After a violent heat for 65 minutes the crucibles were withdrawn from the furnace: when cool, I found a slight crack in the exterior one; the sand however remained entire, and firmly cemented together, though not vitrified, forming a thick porous lute surrounding the small crucible. Upon removing the sand, and examining the interior vessel, I found the pieces of iron fused into one solid mass, which proved soft steel. The mass of sand, which occupied the mouth of the inverted pot, contained several detached globules of steel; these, during the intensity of the heat, and the extreme division of the fluid, had penetrated the sand, and one of them had actually reached the bottom of the outer crucible. These masses

had in their descent conveyed a blackish-blue colour to the sand, and the fusion of the whole had glazed the bottom and sides of the inverted crucible, unoccupied by the sand, with the same colour. The principle button, and small pieces, weighed 1229 grains: loss, 21 grains,  $= \frac{1}{60}$  of the original weight of the iron.

“ The quality of this steel was uncommonly red-short, and it was with difficulty it would take a form under the hammer. When cold it was much tougher than good steel is ever found to be, and distended in this state astonishingly under the hammer. The fusion, however, had been most complete; every part of the result possessed a crystallised surface more or less accurate. Upon the whole, the quality of this steel is, I think, similar to some described lately by C. Clouet. Its uncommon softness, while it possessed not the property of drawing into shape with a smooth uncracked surface, led me to conceive that the fusion was urged too speedily, and before the sufficient quantity of carbon had been taken up which constitutes good steel. This induced me to repeat the experiment in the following manner:—

II. “ Having prepared, in an annealing-furnace, three crucibles, made from Sturbridge clay, of various sizes; into the smallest one I put five pieces of malleable iron weighing 1875 grains; upon the top of which I wedged a high-baked cover of fire-clay: upon the top of this cover, and to a level with the edges of the crucible, I introduced infusible sand. This operation was performed upon a plate of red-hot iron, lest the sudden contact of the cold air should endanger the soundness of the pots. The small crucible was next introduced into the second size, and a cover fitted over the two. Lastly, this double crucible was placed into one so much larger as to admit of a stratum of sand, nearly half an inch thick, in such a manner as to insulate the former most completely. This operation finished by covering the outside crucible with a large fire-clay cover. My reason for leaving the vacant space betwixt the two innermost crucibles not filled up was, to guard against any error in the result, which might probably be occasioned by the action of the interior fire-clay cover, and the sand, in their respective degrees of

shrinkage and expansion. The whole was exposed to a moderate white heat for nearly an hour, and for forty minutes further a heat equal to the whole power of the furnace. The crucibles were safely withdrawn, and, when examined, the following facts presented themselves:—

“ The outer crucible remained entire; the sand, next, formed an entire vessel considerably connected. The second crucible was also found, but in the bottom of it I found an irregular metallic mass. A small hole in the side of the interior pot had allowed nearly one-half of the original weight of iron to pass; the remainder of it, forming a very beautiful circular button, rested upon the bottom. The surface was crystallised in radii, shooting from a central point upon the upper surface of the button. The collective weight was 1858 grains: loss, 17 grains, =  $\frac{1}{100}$  part the weight of the iron. When the metal was lodged in the small crucible, the sides and bottom were glazed, as in the former experiment.

III. “ The same experiment was repeated with bottle glass, and a similar length and degree of heat applied. The reduction of the glass was entire. Betwixt the exterior and middle crucible, the glass appeared as an entire interposed vessel, completely surrounding the two smaller ones. Upon examining the interior crucible, I found that the contact of the bottle-glass had fused the cover of fire-clay fitted over the iron, and that the promiscuous fusion of the glass and clay formed a very fine dark-green transparent mass, beneath which lodged the metallic button. The upper surface still preserved the acuteness of the original angles upon one piece of iron; the others had resolved themselves by fusion into a very fine ingot of soft steel with partially crystallised edges. The weight of the whole was 1759 grains; less than when put in, five grains; equal to  $\frac{1}{37} \frac{4}{5}$  part of the whole.—I am convinced that by prolonging the heat ten minutes, the whole iron would have entered into fusion. I would from this and other experiments infer, that the iron is of more difficult carbonation when a fused medium is made use of, than when either pure sand, pure lime, or clay, are used.

IV. “ In order that no proof might be further wanting to establish the fact of the iron receiving the carbon from its so-

lution in caloric, and to obviate any objection which might probably be started against this, by supposing that the carbon might be conveyed through the porous medium of the earths, or the bottle-glasses, *before* it entered into fusion, I made the following experiment:—Having made a mould in sand, I poured into it thinly fused calcareous stone; and, while the fluid was thin, introduced a small rod of red-hot iron, suspended by a very small brass wire. The dimensions of the rod were so proportioned to the mould that  $\frac{5}{8}$ ths of an inch of a virid crust encircled the iron on all sides. The wire became fused and disengaged as the fluid was consolidating, so that every avenue to the iron was completely shut up. The mass was carefully cooled to prevent shivering, and as carefully heated, when introduced into the furnace, in a crucible inverted in one larger, and the vacancy betwixt each shut up from air by means of pounded bottle-glasses. A cover was fitted on, and a violent heat urged for nearly two hours. The crucibles were taken out, free from blemish, and cemented as one compact mass. When cool, I found the metal in the interior crucible resolved into a very fine ingot, which proved afterwards to be soft steel; capable, however, of hammering and hardening to great advantage. The weight of steel obtained was 1157 grains; lost, 33 grains,  $= \frac{1}{36}$  part of the whole. The fused lime, which had formerly cooled of a pure whitish porcelain colour variegated with blue streaks, was now of a spongy brown colour, exactly resembling the lava of the blast-furnace when oxygenated crude iron is produced. This alteration was unquestionably owing to the deficient weight in metal having in part been oxydated by the interior air of the crucible, or by oxygen from the furnace, and united to the earth in the state of a fused oxyde. The great difference in the loss of metal sustained in these two last experiments must be sought for in the quantity of iron taken up by the flux: in the others, the solidity and transparency of the glass were uncommonly fine; in these the flux was opaque, dark, and porous, evidently furcharged with iron.

“ It will appear from these experiments, that we are still without any satisfactory or conclusive proof of the steelification of iron solely by means of the diamond,

“ There

“ There is one experiment which would, if successful, establish the point. C. Clouet calculates, that, to constitute grey crude iron, 1-6th of carbon is necessary. Were six parts of iron, and a diamond, equal to 1-6th of this, subjected to the same process as formerly, and the result *grey, or carbonated crude iron*, instead of steel, the inference would be just, and the conclusion satisfactory.

“ In the present state of my information, I doubt whether the diamond afforded even one particle of carbon to the iron. From many experiments, wherein I used very pure well charred wood and malleable iron, I uniformly found that the weight of the steelified ingot nearly equalled the sum of that of the iron and carbon originally introduced: so that, in place of having a loss of *real iron* equal to  $\frac{1}{40}$ , as experienced by the Parisian chemists, had the diamond been taken up by the iron, the ingot of steel would have weighed within  $\frac{1}{200}$  part of the aggregate weight of both, and from  $\frac{1}{20}$  to  $\frac{1}{70}$  part more than the iron at first introduced.

“ So far as I have gone, I think my experiments quite conclusive: the others you mention shall be attended to in due time.”

#### MONUMENT TO COUNT RUMFORD.

In the introduction to a German translation of Count Rumford's Essays, lately published at Weimar, we find, among other anecdotes relative to the author, an account of a monument that was erected at Munich in the year 1795 in commemoration of his public services.

This monument, which is situated in a beautiful public garden adjoining to the ramparts of the city, was erected (without the knowledge of the Count, while he was absent from Bavaria,) by the principal inhabitants of Munich, with the permission and approbation of the late Elector.

The monument is a solid pile of a quadrangular form, constructed of hewn stone, about twenty feet in height; and it has two principal fronts, which are ornamented with sculpture and inscriptions.

Upon one of these fronts there is a basso relievo of two figures

figures at whole length representing the Genius of Plenty leading Bavaria by the hand, and strewing her path with flowers. Under this emblematical piece of sculpture, upon a broad tablet of Bavarian marble, there is the following inscription in the German language:—

LUSTWANDLER STEH!

DANK STÆRKET DEN GENUS.

EIN SCHÖPFERISCHER WINK CARL THEODORES,  
VON MENSCHENFREUNDE RUMFORD,  
MIT GEIST. GEFÜHL, UND LIEB' GEFAST,  
HAT DIESE EINMAL'S EDE GEGEND  
IN DAS WAS DU NUN UM DICH SIHEST  
VEREDELT.

Which may be thus translated:

Stay, Rambler!

Thankfulness increases Enjoyment.

The creative glance of Charles Theodore

Rumford, the Friend of Mankind;

With Genius, Taste, and Love inspired,

Changed this once desert place

Into what thou now beholdest.

Upon the opposite front of the monument there is a medallion of Count Rumford, as large as the life, reckoned a good likeness, and under it the following inscription:—

IHM

DER DAS SCHMAEHFLICHSTE OEFFENTLICHER UEBEL,

DEN MUESSIGANG UND BETTEL TILGTE;

DER ARMUTH HILF, GEWERB UND SITTEN,

DER VATERLAENDISCHEN IUGEND

SO MANCHE BILDUNGSANSTALT GAB:

LUSTWANDLER GEH!

UND SINNE NACH IHM GLEICH ZU SEIN

AN GEIST UND THAT,

UND UNS

AN DANK.



In English as follows :—

To Him  
Who rooted out the most disgraceful of public Evils,  
Idleness and Mendicity ;  
Who relieved and instructed the Poor,  
And founded many Institutions  
For the Education of our Youth :  
Go, Wanderer !  
Strive to equal him  
In Genius and Activity,  
And us  
In Gratitude !

That Count Rumford may long live to enjoy the heart-felt satisfaction that must result from the contemplation of the success of his labours in promoting the happiness of mankind, must be the sincere wish of every lover of science, and of every true friend to virtue and morality,

LUMINOUS APPEARANCE PRODUCED BY SUGAR.

In the process of breaking fine white sugar in a dark apartment, a very perceptible luminous appearance is observed, the cause of which is as much unknown to us as that produced by rubbing against each other two flints, particularly those of Iceland. The following is offered by Dr. Juch\*, of Würzburg, as a kind of explanation of this phenomenon:

“ While breaking refined sugar,” says he, “ in small pieces with a hammer and blunt knife, I suddenly perceived a very singular smell, which had a great resemblance to that of fuming nitrous acid. I looked immediately round me to see whether some vessel containing that acid might not be in the neighbourhood; but I saw none, and was certain that nothing of the kind had been in the room. I then examined the circumstance more narrowly, and found, on breaking fine sugar, that a smell similar to that of aquafortis was always perceived by every person present.

“ This simultaneous disengagement of light, and of the smell of nitrous acid, I explain in the following manner:—When

\* In *Scheerer's Journal der Chemie*, No. 10.

sugar is broken, a new surface being presented to the air, a real acidifying process instantly takes place: a small portion of the oxygen of the atmosphere unites itself with the sugar, but not enough to form saccharine acid, as the quantity is not perhaps sufficient for that purpose; and at the same moment the atmosphere in contact with the new surface of the sugar is so changed, in regard to the proportion of its component parts, that it affects the olfactory organs in the same manner as nitrous acid. This phenomenon may receive some farther illustration by repeating the experiment in a close vessel."

#### A SINGULAR OXYDATION OF IRON.

Dr. Juch also gives the following singular notice:—

"I had," says he, "a small leather flask filled with iron filings, which I frequently used with my electrophorus both charged and uncharged. I had occasion one day for a quantity of pure iron, and, having no other at hand, had recourse to the filings in the flask, which I knew were pure. I emptied it on a piece of paper. I laid the paper on my hand in order to convey it to the place of its destination; but I had scarcely held it a few seconds when I perceived a strong heat, which increased so much that I could no longer hold the paper. Some minutes after, the paper became brown, and at length took fire. It did not, however, flame. The iron filings were in a state of ignition; and when the heat had decreased, I found the iron converted into a highly friable oxyd."

#### ELECTRICITY OF ICE.

The same author mentions, that electric sparks may be obtained by the usual manipulation from a cylinder of ice. Those who may not be afraid of the labour, can make the experiment at the temperature of  $-16\frac{1}{2}$  R. =  $-5\cdot125$  Fahr.

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THE  
PHILOSOPHICAL MAGAZINE.

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DECEMBER 1799.

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I. *On different Sorts of Lime used in Agriculture.* By  
SMITHSON TENNANT, Esq. F.R.S.\*

I WAS informed last summer, that, in the neighbourhood of Doncaster, two kinds of lime were employed in agriculture, which were supposed to differ materially in their effects. One of these, which was procured near the town, it was necessary to use sparingly, and to spread very evenly over the land: for it was said, that a large proportion of it, instead of increasing, diminished the fertility of the soil; and that, wherever a heap of it was left in one spot, all vegetation was prevented for many years. Fifty or sixty bushels upon an acre, were considered to be as much as could be used with advantage. The other sort of lime, which was obtained from a village near Ferry-bridge, though considerably dearer from the distant carriage, was more frequently employed, on account of its superior utility. A large quantity was never found to be injurious; and the spots which were entirely covered with it, instead of being rendered barren, became remarkably fertile. The different properties ascribed to these two kinds of lime were so very distinct, that it seemed probable they could not be imaginary; and it therefore appeared to be worth the trouble of ascertaining them more fully, and

\* From the *Phil. Transf. of the Royal Society* for 1799, Part II.

of attempting to discover the nature of the ingredients from whence the difference arose. For this purpose I procured some pieces of each sort of limestone, and first tried what would be their effect upon vegetables, in their natural state, by reducing them to coarse powder, and sowing in them the seeds of different plants. In both kinds the seeds grew equally well, and nearly in the same manner as they would in sand, or any other substance which affords no nourishment to vegetables. Pieces of each sort of stone were then burnt to lime; and, after they had been exposed for some weeks to the air, that their causticity might be diminished, some seeds were sown in them. In the kind of lime which was found most beneficial to land, almost all the seeds came up, and continued to grow as long as they were supplied with water; and the roots of the plants had many fibres, which had penetrated to the bottom of the cup in which they grew. Upon examining the composition of this sort of lime, it proved to consist entirely of calcareous earth. By its exposure to the air for about three months, it was found to have absorbed 4-5ths of the fixed air required to saturate it. In the other kind, a few only of the seeds grew, and the plants produced from them had hardly any stalks or roots, being formed almost entirely of the two seed-leaves, which lay quite loose upon the surface. This sort of lime, being spread upon a garden soil, to the thickness of about the tenth of an inch, prevented nearly all the seeds which had been sown from coming up, whilst no injury was occasioned by common lime used in the same manner. Upon examining the composition of this substance, which was so destructive to the plants, it was discovered to contain three parts of pure calcareous earth, and two of magnesia. The quantity of fixed air which it had absorbed, by being exposed for about the same time as the pure lime just mentioned, was only 42 hundredths of that combined with it before it was burnt.

As it seemed probable that the magnesia contained in this lime was the cause of its peculiar properties, the following experiments were made, to determine the effects of that substance upon the growth of vegetables. Some seeds, chiefly of colewort, which were preferred from their growing quickly,

were

were sown in uncalcined magnesia; but, though they sprouted, the leaves never rose above the surface, and the plants were entirely without roots, nor did they appear to grow better in magnesia which had been washed in water containing fixed air. Calcined magnesia was, however, much more destructive, as the seeds would not come up in it. To compare its effects on vegetables with those of lime, each of these earths was mixed, in different proportions, with sand, in small cups, in which seeds were then sown. The lime was obtained from marble; and, before it was put into the sand, was made to fall to powder, by being moistened with water. In a mixture of four ounces of sand with three or four grains of calcined magnesia, it was a long time before the seeds came up, and the plants had hardly any roots or stalks; and, with ten grains or more of magnesia, there was no appearance of vegetation. Thirty or forty grains of lime did not retard the growth of the seeds more than three or four of magnesia, and the injurious effects were not so lasting. The lime, by absorbing fixed air, soon lost its destructive properties; so that, after keeping these mixtures four or five weeks, seeds were found to grow in that with forty grains of lime, nearly as well as in pure sand; but, in that with four grains of magnesia, they produced only the seed-leaves, as was described before. It was necessary occasionally to break in pieces the sand which had so much lime, as it would otherwise have been too hard to admit the seeds to penetrate through it. Plants will bear a much larger proportion of magnesia in vegetable soil than in sand: with twenty grains, however, of calcined magnesia, in as much soil as was equal in bulk to four ounces of sand, the seeds produced only the seed-leaves without roots; and, with about forty grains, they were entirely prevented from coming up.

In countries where the magnesian lime is employed, it was said, that the barrenness of any spot on which a heap of it had been laid, would continue for many years. To learn how far it could by time be deprived of its injurious qualities, I procured some pieces of mortar, made of this species of lime, from two houses, one of which had been built three, and the other eight years: they were taken from the outside

of the building, where they had been exposed to the air. After they were reduced to powder, seeds were sown in them. Only a few came up, and even those produced merely the seed-leaves, without any roots. As plants would grow in the limestone from which this species of lime was formed, although not in the mortar made from it, I wished to know what proportion of the fixed air, originally contained in the limestone, had been absorbed by the mortar. For this purpose a piece of it was finely powdered, to render it of an uniform quality: it was then tried how much of this powder, and of the limestone, would saturate the same quantity of acid: by this means I ascertained the proportions of limestone and mortar, containing equal quantities of the magnesian lime. The fixed air being obtained from them in those proportions, and measured in an inverted vessel, with quicksilver, it was found that the mortar which had been exposed three years had absorbed 43, and that of eight years only 47 hundredths of the quantity originally contained in the limestone. I was not able to obtain any mortar which had been made earlier, though it might deserve to be known how much fixed air it was ultimately capable of absorbing. Common mortar, which had been exposed to the air for a year and three quarters, had regained 63 hundredths of its full quantity of fixed air.

As the preceding experiments were tried during the winter, in a room warmed by fire, perhaps under circumstances more favourable to vegetation, the same quantity of magnesia would not be equally pernicious.

Magnesian limestone may be easily distinguished from that which is purely calcareous by the slowness of its solution in acids, which is so considerable, that even the softest kind of the former is much longer in dissolving than marble. From this property of the magnesian limestone, there appeared to be reason for suspecting that the kind of marble which had been called Dolomite, from M. Dolomieu, who first remarked its peculiarity in dissolving slowly, might also be similar in its composition. An analysis of this substance was lately given in the *Journal de Physique*: but this is probably erroneous; for, upon examining three specimens, they were

were found to consist of magnesia and calcareous earth, like the magnesian limestone; so that it ought no doubt to be considered as the same species of stone, but in a state of greater purity. The pieces of dolomite were from different places; one of them being found among the ruins of Rome, where it is thought to have come from Greece, as many statues of Grecian workmanship are made of it, and no quarries of a similar kind are known in Italy; the second was said to have been thrown up by Mount Vesuvius; and the third was from Iona, one of the western islands of Scotland. In many kinds of common marble, small particles and veins may be observed, which are a long time in dissolving. These, upon examination, I discovered to contain a considerable proportion of magnesia; but, as they were probably not quite free from the surrounding marble, I did not ascertain the quantity precisely.

The crystallised structure which may generally be observed in the magnesian limestone, seems to shew that it has not been formed by the accidental union of the two earths, but must have resulted from their chemical combination. The difficulty of dissolving it, may also arise from the attraction of the different component parts to each other. The mortar formed from this kind of lime is as soluble in acids as common marble; and the substances of which it consists are easily separated. The magnesia may be taken from it by boiling it in muriated lime, and lime is precipitated by it from lime water; but neither of these effects can be produced by the stone before it is calcined.

Magnesian limestone is probably very abundant in various parts of England. It appears to extend for thirty or forty miles, from a little south-west of Worktop in Nottinghamshire, to near Ferry-bridge in Yorkshire. About five or six miles further north there is a quarry of it, near Sherburn; but whether this is a continuation from the stratum near Ferry-bridge, I have not learnt. From some specimens which were sent me, I find that the cathedral and walls of York are made of it. I have not been able to learn whether there were any shells in the limestone of the tract of country before-mentioned. In Mr. Marshall's account of the agriculture of the midland counties, he speaks of the lime made at Breedon, near Derby, as destructive to vegetables, when used  
in

in large quantities. I therefore procured some pieces of it, and they were discovered to contain nearly the same proportion of magnesia as that before described. In this quarry the stone is frequently crystallised in a rhomboidal form; and petrified shells, not calcareous, but similar in composition to the stone itself, are sometimes, but very rarely, found in it. This substance seems to be common in Northumberland. In the third volume of the *Annals of Agriculture*, Dr. Fenwick, of Newcastle, observes, that the farmers of that country divide limes into hot and mild. The former of these is no doubt magnesian, as it has similar effects on the soil; and he remarks, that it is not so easily dissolved in acids as the latter. At Matlock, in Derbyshire, the two kinds are contiguous to each other; the rocks on the side of the river where the houses are built being magnesian, and on the other calcareous. The magnesian rock appears also to be incumbent upon a calcareous stratum; for, in descending a cave formed in this rock, a distinct vein of common limestone may be observed, which contains no magnesia. The latter stratum is very full of shells; but, though there are some also in the magnesian rock, yet they are very rare. In the following tables, containing the analysis of various specimens, some other places are mentioned where this substance is found, but of which I received no further information.

After it was known that the magnesian marble and limestone consisted of the two earths, their proportion was attempted to be discovered, by trying how much gypsum and Epsom salt could be obtained, by means of vitriolic acid, from a certain weight of each specimen. When the superfluous vitriolic acid had been evaporated by heat, the Epsom salt was separated from the gypsum by water. The result of these trials is expressed in the following table:—

	Dry Gypsum.	Dry Epsom Salt.
5 grains of limestone from Breedon gave	3.9	3.15
————— Matlock -	3.95	2.9
————— Workfop -	3.8	3.0
————— York -	3.8	3.1
3 grains of calcareous spar, and 1 grain of calcined magnesia, gave	3.9	2.7



As the preceding method of estimating the quantities of magnesia and calcareous earth is liable to considerable error, I afterwards examined them in the following manner, which seems capable of great exactness:—Twenty-five grains of each substance were dissolved, by marine acid, in a cup of platina, and, after the solution was evaporated to dryness, it was made red-hot for a few minutes. The mass remaining in the cup, which consisted of muriated lime, and of the magnesia freed from the acid, was washed out with water, and poured into a phial. There was then added to it a known quantity of diluted marine acid, somewhat more than was sufficient to redissolve the magnesia, and, after the solution, a certain weight of calcareous spar, part of which would be dissolved by the superfluous acid. By the quantity of spar remaining undissolved, it was learnt how much acid was required to dissolve the magnesia. The iron and argillaceous earth contained in some specimens, were precipitated by the spar, and therefore could not occasion any error. The calcareous spar, however, dissolved more slowly where there was argillaceous earth, as it became coated with it; but this incrustation was occasionally removed, and, in all the experiments, the spar was left in the solution till it suffered no further diminution. For this purpose it was necessary to keep them slightly warm for some days, during which time the phials were generally closed, to prevent any escape of the acid.

The first experiment in the following table was made upon known quantities of magnesia and calcareous earth, to try the accuracy of the process. For this purpose, also, the second was repeated upon a piece of limestone, previously powdered, to render every part of it of the same quality. The first column shews the quantity of calcareous spar which might have been dissolved by the acid required to take up the magnesia. The second shews the corresponding quantities of magnesia in 25 grains of each substance. The third expresses the quantity of lime. This was inferred by subtracting the weight of the magnesia, and of the iron and clay, from 13.2 grains, the weight of the whole quantity of earth in 25 grains of limestone. This is probably not very  
incorrect,

incorrect, as, in two specimens which differed most in the proportion of magnesia and lime, the weight of the two earths was nearly the same.

A piece of dolomite, from Rome, was wrapped in a thin leaf of platina, that no part of it might be lost, and, being then exposed to a strong heat, left of earth - 52.9 *per cent.*

Dolomite from Mount Vesuvius - 52.8

Breedon limestone - 52.4

Calcareous spar left of lime - 55.8

In three of the experiments, also, the calcareous earth was precipitated by mineral alkali; and the quantity of it being tried by that of the marine acid required to dissolve it, it corresponded very nearly with that put down.

A quantity of marine acid which would dissolve 15 grains of calcareous spar, would also dissolve 5.5 of calcined magnesia, and 2.5 grains of spar; so that 12.5 grains of spar, required the same quantity of acid as 5.5 grains of magnesia.

The magnesia used was very pure, and made red-hot immediately before it was weighed.

<i>Substances examined.</i>	Quantity of spar which the acid, required to take up the magnesia, would have dissolved.	Quantity of magnesia.	Quantity of lime.	Iron and clay.
Mixture of 5.5 grains of magnesia and 14 grains of calcareous spar - - -	12.5	5.5	7.8	0
25 grains of Breedon limestone, previously powdered - -	11.53	5.071	7.929	.2
25 grains from part of the same powder - - -	11.56	5.082	7.913	.2
25 grains of dolomite from Rome - - -	12.2	5.37	7.73	.1
----- dolomite from Iona - - -	10.1	4.4	7.8	1.0
----- Vesuvian dolomite - - -	10.38	4.565	8.575	.06
A second experiment, from part of the same Vesuvian dolomite - - -	10.03	4.411	8.849	.06
25 grains of magnesian limestone from Wanfworth, near Doncaster - - -	12.75	5.61	7.34	.25
----- Thorpe arch - -	10.95	4.84	7.8	.6
----- Matlock - -	12.5	5.5	7.388	.31
----- York-minster - -	11.	4.84	8.26	.1
----- Workfop - -	11.6	5.104	7.496	.6
----- Sherburn - -	11.5	5.08	7.56	.56
----- Westminster-hall	10.1	4.4	8.37	.4

II. *Agenda, or a Collection of Observations and Researches, the Results of which may serve as the Foundation for a Theory of the Earth.* By M. DE SAUSSURE.

[Concluded from Page 140.]

CHAP. XXIII.

*Instruments necessary for the Geological Traveller.*

**T**HE most necessary instrument is the miner's hammer. It will be requisite to have two, of different sizes: one small, to break small fragments of rolled pebbles, by holding them in the left hand while you strike with the right. Its weight, including the handle, ought to be about ten ounces. The other must be larger, to detach fragments of rock, and to break large pebbles: its weight ought to be quadruple that of the small one. When I travel on horseback, I have these two hammers suspended from the bow of my saddle.

1. A. Two stone-cutter's chisels: one small, of from a line to a line and a half, to detach small crystals, or other objects of small bulk; the other, seven or eight lines.

2. To try the hardness of fossils, a piece of steel to strike fire will be necessary; also a triangular file, pretty fine, and a strong bodkin of tempered steel.

3. Nitrous acid, with M. De Morveau's boxes of reagents.

3. A. An artificial magnet, in a case, with a steel pivot on which it can be placed, to try the magnetism of fossils.

4. A magnifying glass of three inches focus, in order to enable the observer to form a general idea of any fossil: another, of an inch focus, to examine its separated parts; and one of five or six lines for closer examination. These three magnifiers must be always in the traveller's pocket, or ready at hand: but, besides these, he must have, for his closet at home, a microscope furnished with a micrometer.

• 5. Telescopes, to observe inaccessible mines and distant mountains.

6. A pocket portfolio, with prepared paper for writing on.

with a pencil of tin folder, which it is not necessary always to cut, and the writing of which is not so easily effaced as that of plumbago. In this portfolio the traveller must write out, on the spot, the sketch of his journal, and insert such observations as occur to him; but he must take the trouble to transcribe these notes at more length, preserving the primitive notes, which will always retain a character of truth, and for that reason people are fond of recurring to them.

7. Some quires of brown paper, a few sheets of which may be carried in the pocket for wrapping up on the spot specimens of the stones you collect, the characters of which ought to be marked on the cover. You may afterwards pack them with hay into a bag destined for that purpose, until you have a sufficient quantity to form a box, which you may send home by the public carriages wherever you find an opportunity; but, as it is fatiguing for the traveller to load his pockets during the time of his excursions, and as the guides often lose them on purpose in order to get rid of them, I have behind my saddle two leathern bags, into which I put them till I come to some halting-place, where I have time to pack them with hay into a bag. M. Besson recommends to those who undertake sea voyages to write with China ink the characters which ought to accompany minerals in long passages, because common ink may be effaced by accidents.

8. A blow-pipe, with its apparatus. As I make much use of this instrument, which at length fatigues me, though I can blow with my cheeks without exercising my breast, I caused to be constructed a pair of portable double bellows, the sides of which contain each sixty-two square inches. These bellows can be suspended from the edge of a table; and I put them in motion by pressing together, between my knees, the two handles, which afterwards separate by the action of the spring. This apparatus may be easily carried, and is very convenient.

9. A graduated semicircle traced out on a copper-plate of a form exactly rectangular, with a plummet suspended from the centre of the semicircle. This semicircle is the most convenient instrument for measuring the inclination of strata, of veins and declivities; and it may always be carried in a pocket of the portfolio.

10. A compass, furnished with a cross staff, to find the direction of mountains, chains, vallies, and strata.

11. Portable barometers with two mercurial thermometers; one affixed to a barometer to estimate the temperature of the mercury in the latter, and the other with a bare bulb for measuring the temperature of the air. Those who study meteorology, as well as geology, ought to be furnished also with an hygrometer and an electrometer.

12. For ascertaining the temperature of the sea, at great depths, it will be necessary to have a thermometer constructed like that described in my Travels through the Alps\*: for lakes, an apparatus like that pointed out in the note of Section 1399; will be sufficient.

13. Those who understand a little geometry, ought to provide themselves with a sextant, having an artificial horizon, and also a chain, in order that they may be able to measure a base, and thus take the altitude of an inaccessible peak, the breadth of a river, &c. &c. With this sextant they may also find the latitudes. In regard to the longitudes, they require, besides instruments, an expertness in this kind of observation, which cannot be attained but by mariners or professed astronomers.

14. It will be necessary also to have within reach tools for repairing an instrument in case it should happen to be deranged; such as pincers, files, turnscraws, compasses, gimblets, wire, needles, thread, and packthread.

15. Lastly, some good map, pasted on canvas, of the country you propose to examine; and this map ought frequently to be compared with your itinerary, and the bearings given by your compass.

16. In regard to the care required for the traveller's person, he must have a light dress made of cloth, without lining, of a white colour, as well as his hat, that he may be less exposed to the heat of the sun's rays; with jackets, some cool for the warm regions and the vallies, and the other warm for the cool regions and eminences; a good great coat; green spectacles, and a black crape, to secure the eyes and face from the snow. Lastly, if he is to pass the night in the open air, a

\* Section 1202, Plate 1, fig. 3.

tent or *cannonicere*, a bear's skin to sleep upon, and woollen blankets.

17. A solid light walking-pole: mine for the high Alps is a young fir-plant, extremely dry, seven feet in length and 18 lines in diameter at the lower end, which is shod with iron tapering to a strong point. These dimensions may appear large, but nothing can be too strong for the steep rocks, glaciers, and snow, when you are obliged to take your point of support at a distance from you, and to rest the whole weight of your body on your pole, by holding it in a situation very much inclined, and even horizontal, as may be seen in the Vignette to the First Volume of my Travels through the Alps. —For mountains which are not so steep, the traveller may be satisfied with poles of less strength and size; but it will still be necessary that they should be four or five feet in height, and sufficiently strong that a person might be able to support himself with his two hands by holding them in a horizontal situation, according to the attitude of the small figure which is on the left side at top of the before-mentioned vignette; for, in traversing or descending a rapid declivity, or in walking on the margin or edge of a precipice, the traveller must always support himself by his two hands, holding the pole towards the mountain, and not towards the precipice, as those do who have not learned the art of travelling through mountains.

18. To prevent slipping on the hard snow-ice, and grass-plats, which are still more dangerous, I would recommend iron cramps, such as those which I have caused to be engraved in the third plate of the first volume, and which I have long used with success. In my last excursions, however, I preferred shoes, the thick soles of which were armed with strong tacks, at the distance of eight or nine lines from each other. The heads of these tacks are of steel, and shaped like a square pyramid: I have some small ones, the points of which are only two lines and a half in height, and of about the same breadth, for the snow-rocks and grass-plats; and others, of double these dimensions, for the hard snow.

19. In the last place, with regard to provisions, when the traveller must reside for a considerable time in the deserts, at a distance from habitations, and even huts, he may carry  
with

with him some salt or pickled meat; but M. Parmentier's saloop of potatoes, with cakes of portable soup, and bread, will form the most nourishing food, and what may be contained in the least room. A small iron chaffing-dish, a small bag filled with charcoal, and a pan of tinned copper or iron, form my kitchen apparatus for the mountains: wooden plates and spoons may be found in the remotest huts. It will be proper, however, to carry always in the pocket a cup of gum elastic, in order that the traveller may at all times easily quench his thirst, a want to which he will be frequently exposed in his excursions.

From what has been said, it may be readily seen, that the study of geology will not suit the indolent or sensual; for the life of the geologue must be divided between fatiguing and perilous journies, in which he is deprived of almost all the conveniences of life, and the varied and deep researches of the closet. But what is still more rare, and perhaps more necessary than the zeal requisite to surmount these obstacles, is, a mind free from prejudice, filled with an ardent desire for the truth alone, rather than with a desire for raising or destroying systems; capable of descending to minute details indispensibly necessary for the correctness and certainty of observations, and of rising to grand views and general conceptions. Those fond of such studies, ought not, however, to be discouraged by these difficulties; there is no traveller who may not make some good observation, and bring with him at least one stone worthy of being employed in the construction of this grand edifice. It is indeed possible to be useful without attaining to perfection; for I have no doubt that if the mineralogical travels, even the most esteemed, and much more those of the author, be compared with these *Agenda*, there will be found in them many deficiencies, and many observations, either imperfect, or even totally forgotten: but I have mentioned the reason in the Introduction. Besides, several of these ideas did not occur till I had finished my travels; and for that reason I laboured with more zeal on these *Agenda*, in the hope of rendering young persons, on their entering this career, capable of performing what cost me thirty-six years of travelling and study.

III. *On the Nature and Construction of the Sun and Fixed Stars.*  
By WILLIAM HERSCHEL, LL.D. F.R.S.

[Concluded from Page 123.]

IT will now be easy to bring the result of these observations into a very narrow compass. That the sun has a very extensive atmosphere cannot be doubted; and that this atmosphere consists of various elastic fluids, that are more or less lucid and transparent, and of which the lucid one is that which furnishes us with light, seems also to be fully established by all the phænomena of its spots, of the faculæ, and of the lucid surface itself. There is no kind of variety in these appearances but what may be accounted for with the greatest facility, from the continual agitation which, we may easily conceive, must take place in the regions of such extensive elastic fluids.

It will be necessary, however, to be a little more particular as to the manner in which I suppose the lucid fluid of the sun to be generated in its atmosphere. An analogy that may be drawn from the generation of clouds in our own atmosphere, seems to be a very proper one, and full of instruction. Our clouds are probably decompositions of some of the elastic fluids of the atmosphere itself, when such natural causes, as in this grand chemical laboratory are generally at work, act upon them: we may therefore admit, that in the very extensive atmosphere of the sun, from causes of the same nature, similar phænomena will take place; but with this difference, that the continual and very extensive decompositions of the elastic fluids of the sun are of a phosphoric nature, and attended with lucid appearances, by giving out light.

If it should be objected, that such violent and unremitting decompositions would exhaust the sun, we may recur again to our analogy, which will furnish us with the following reflections. The extent of our own atmosphere, we see, is still preserved, notwithstanding the copious decompositions of its fluids in clouds and falling rain; in flashes of lightning, in meteors, and other luminous phænomena; because there are fresh supplies of elastic vapours continually ascending to make



good the waste occasioned by those decompositions. But it may be urged, that the case with the decomposition of the elastic fluids in the solar atmosphere would be very different, since light is emitted, and does not return to the sun, as clouds do to the earth when they descend in showers of rain. To which I answer, that, in the decomposition of phosphoric fluids, every other ingredient but light may also return to the body of the sun. And that the emission of light must waste the sun, is not a difficulty that can be opposed to our hypothesis: for, as it is an evident fact that the sun does emit light, the same objection, if it could be one, would equally militate against every other assignable way to account for the phenomenon.

There are, moreover, considerations that may lessen the pressure of this alledged difficulty. We know the exceeding subtilty of light to be such, that in ages of time its emanation from the sun cannot very sensibly lessen the size of this great body. To this may be added, that, very possibly, there may also be ways of restoration to compensate for what is lost by the emission of light, though the manner in which this can be brought about should not appear to us. Many of the operations of Nature are carried on in her great laboratory which we cannot comprehend, but now and then we see some of the tools with which she is at work. We need not wonder that their construction should be so singular as to induce us to confess our ignorance of the method of employing them, but we may rest assured that they are not a mere *lufus nature*. I allude to the great number of small telescopic comets that have been observed, and to the far greater number still that are probably much too small for being noticed by our most diligent searchers after them. Those six, for instance, which my sister has discovered, I can from examination affirm, had not the least appearance of any solid nucleus, and seemed to be mere collections of vapours condensed about a centre. Five more, that I have also observed, were nearly of the same nature. This throws a mystery over their destination, which seems to place them in the allegorical view of tools, probably designed for some salutary purposes to be wrought by them; and, whether the restoration of what is  
lost

lost to the sun by the emission of light, the possibility of which we have been mentioning above, may not be one of these purposes, I shall not presume to determine. The motion of the comet, discovered by Mr. Messier in June 1770, plainly indicated how much its orbit was liable to be changed by the perturbations of the planets; from which, and the little agreement that can be found between the elements of the orbits of all the comets that have been observed, it appears clearly that they may be directed to carry their salutary influence to any part of the heavens.

My hypothesis, however, as before observed, does not lay me under any obligation to explain how the sun can sustain the waste of light, nor to shew that it will sustain it for ever; and I should also remark that, as in the analogy of generating clouds, I merely allude to their production as owing to a decomposition of some of the elastic fluids of our atmosphere, that analogy, which firmly rests upon the fact, will not be less to my purpose, to whatever cause these clouds may owe their origin. It is the same with the lucid clouds, if I may so call them, of the sun. They plainly exist, because we see them; the manner of their being generated may remain an hypothesis—and mine, till a better can be proposed, may stand good; but, whether it does or not, the consequences I am going to draw from what has been said will not be affected by it.

Before I proceed I shall only point out, that, according to the above theory, a dark spot in the sun is a place in its atmosphere which happens to be free from luminous decompositions; and that faculae are, on the contrary, more copious mixtures of such fluids as decompose each other. The penumbra, which attends the spots, being generally depressed, more or less, to about half way between the solid body of the sun and the upper part of those regions in which luminous decompositions take place, must of course be fainter than other parts. No spot favourable for taking measures having lately been on the sun, I can only judge, from former appearances, that the regions in which the luminous solar clouds are formed, adding thereto the elevation of the faculae, cannot be less than 1843, nor much more than 2765 miles

miles in depth. It is true that in our atmosphere the extent of the clouds is limited to a very narrow compass; but we ought rather to compare the solar ones to the luminous decompositions which take place in our *aurora borealis*, or luminous arches which extend much farther than the cloudy regions. The density of the luminous solar clouds, though very great, may not be exceedingly more so than that of our *aurora borealis*. For, if we consider what would be the brilliancy of a space two or three thousand miles deep, filled with such corruscations as we see now and then in our atmosphere, their apparent intensity, when viewed at the distance of the sun, might not be much inferior to that of the lucid solar fluid.

From the luminous atmosphere of the sun I proceed to its opaque body, which, by calculation from the power it exerts upon the planets, we know to be of great solidity; and from the phænomena of the dark spots, many of which, probably on account of their high situations, have been repeatedly seen, and otherwise denote inequalities in their level, we surmise that its surface is diversified with mountains and vallies.

What has been said enables us to come to some very important conclusions, by remarking, that this way of considering the sun and its atmosphere removes the great dissimilarity we have hitherto been used to find between its condition and that of the rest of the great bodies of the solar system.

The sun, viewed in this light, appears to be nothing else than a very eminent, large, and lucid planet, evidently the first, or, in strictness of speaking, the only primary one of our system, all others being truly secondary to it. Its similarity to the other globes of the solar system with regard to its solidity, its atmosphere, and its diversified surface, the rotation upon its axis, and the fall of heavy bodies, leads us on to suppose that it is most probably also inhabited, like the rest of the planets, by beings whose organs are adapted to the peculiar circumstances of that vast globe.

Whatever fanciful poets might say in making the sun the abode of blessed spirits, or angry moralists devise in pointing it out as a fit place for the punishment of the wicked, it does

not appear that they had any other foundation for their assertions than mere opinion and vague surmise; but now I think myself authorised, *upon astronomical principles*, to propose the sun as an inhabitable world; and am persuaded that the foregoing observations, with the conclusions I have drawn from them, are fully sufficient to answer every objection that may be made against it.

It may, however, not be amiss to remove a certain difficulty, which arises from the effect of the sun's rays upon our globe. The heat which is here, at the distance of 95 millions of miles, produced by these rays, is so considerable, that it may be objected, that the surface of the globe of the sun itself must be scorched up beyond all conception.

This may be very substantially answered by many proofs drawn from natural philosophy, which shew that heat is produced by the sun's rays only when they act upon a caloric medium; they are the cause of the production of heat, by uniting with the matter of fire which is contained in the substances that are heated; as the collision of flint and steel will inflame a magazine of gunpowder, by putting all the latent fire it contains into action. But an instance or two of the manner in which the solar rays produce their effect, will bring this home to our most common experience.

On the tops of mountains of a sufficient height, at an altitude where clouds can very seldom reach to shelter them from the direct rays of the sun, we always find regions of ice and snow. Now, if the solar rays themselves conveyed all the heat we find on this globe, it ought to be hottest where their course is least interrupted. Again, our aëronauts all confirm the coldness of the upper regions of the atmosphere; and since, therefore, even on our earth, the heat of any situation depends upon the aptness of the medium to yield to the impression of the solar rays, we have only to admit, that, on the sun itself, the elastic fluids composing its atmosphere, and the matter on its surface, are of such a nature as not to be capable of any excessive affection from its own rays: and, indeed, this seems to be proved by the copious emission of them; for if the elastic fluids of the atmosphere, or the matter contained on the surface of the sun, were of such a nature

as to admit of an easy, chemical combination with its rays, their emission would be much impeded.

Another well known fact is, that the solar focus of the largest lens, thrown into the air, will occasion no sensible heat in the place where it has been kept for a considerable time, although its power of exciting combustion, when proper bodies are exposed, should be sufficient to fuse the most refractory substances\*.

It will not be necessary to mention other objections, as I can think of none that may be made but what a proper consideration of the foregoing observations will easily remove; such as may be urged from the dissimilarity between the luminous atmosphere of the sun and that of our globe will be touched upon hereafter, when I consider the objections that may be assigned against the moon's being an inhabitable satellite.

I shall now endeavour, by analogical reasonings, to support the ideas I have suggested concerning the construction and purposes of the sun; in order to which it will be necessary to begin with such arguments as the nature of the case will admit, to shew that our moon is probably inhabited. This satellite is of all the heavenly bodies the nearest, and therefore most within the reach of our telescopes: accordingly we find, by repeated inspection, that we can with perfect confidence give the following account of it:—

It is a secondary planet, of a considerable size; the surface of which is diversified, like that of the earth, by mountains and vallies. Its situation with respect to the sun is much like that of the earth, and, by a rotation on its axis, it enjoys an agreeable variety of seasons, and of day and night. To the moon our globe will appear to be a very capital satellite, undergoing the same regular changes of illuminations as the moon does to the earth. The sun, the planets, and the starry constellations of the heavens, will rise and set there as they do here; and heavy bodies will fall on the moon as they do

\* The subject of light and heat has been very ably discussed by Mr. De Luc, in his excellent work, *Idées sur la Météorologie*, Tome I. part 2. chap. 2. section 2. *De la Nature du Feu*; and Tome II. part 3. chap. 6. section 2. *Des Rapports de la Lumière avec la Chaleur dans l'Atmosphère*.

on the earth. There seems only to be wanting, in order to complete the analogy, that it should be inhabited like the earth.

To this it may be objected, that we perceive no large seas in the moon: that its atmosphere (the existence of which has even been doubted by many) is extremely rare, and unfit for the purposes of animal life: that its climates, its seasons, and the length of its days, totally differ from ours: that without dense clouds (which the moon has not), there can be no rain; perhaps no rivers, no lakes. In short, that, notwithstanding the similarity which has been pointed out, there seems to be a decided difference in the two planets we have compared.

My answer to this will be, that that very difference which is now objected will rather strengthen the force of my argument than lessen its value: we find, even upon our globe, that there is the most striking difference in the situation of the creatures that live upon it. While man walks upon the ground, the birds fly in the air, and fishes swim in water; we can certainly not object to the conveniences afforded by the moon, if those that are to inhabit its regions are fitted to their conditions as well as we on this globe are to ours. An absolute, or total sameness, seems rather to denote imperfections, such as Nature never exposes to our view; and, on this account, I believe the analogies that have been mentioned fully sufficient to establish the high probability of the moon's being inhabited like the earth.

To proceed, we will now suppose an inhabitant of the moon, who has not properly considered such analogical reasonings as might induce him to surmise that our earth is inhabited, were to give it as his opinion that the use of that great body, which he sees in his neighbourhood, is to carry about his little globe, that it may be properly exposed to the light of the sun, so as to enjoy an agreeable and useful variety of illumination, as well as to give it light by reflection from the sun when direct day-light cannot be had. Suppose also that the inhabitants of the satellites of Jupiter, Saturn, and the Georgian planet, were to look upon the primary ones, to which they belong, as mere attractive centres, to keep together their orbits, to direct their revolution round the sun, and

to supply them with reflected light in the absence of direct illumination. Ought we not to condemn their ignorance, as proceeding from want of attention and proper reflection? It is very true that the earth, and those other planets that have satellites about them, perform all the offices that have been named for the inhabitants of these little globes; but to us, who live upon one of these planets, their reasonings cannot but appear very defective, when we see what a magnificent dwelling-place the earth affords to numberless intelligent beings.

These considerations ought to make the inhabitants of the planets wiser than we have supposed those of their satellites to be. We surely ought not, like them, to say “the sun” (that immense globe, whose body would much more than “fill the whole orbit of the moon) is merely an attractive “centre to us.” From experience we can affirm, that the performance of the most salutary offices to inferior planets is not inconsistent with the dignity of superior purposes; and, in consequence of such analogical reasonings, assisted by telescopic views, which plainly favour the same opinion, we need not hesitate to admit that the sun is richly stored with inhabitants.

This way of considering the sun is of the utmost importance in its consequences. That stars are suns can hardly admit of a doubt. Their immense distance would perfectly exclude them from our view, if the light they send us were not of the solar kind. Besides, the analogy may be traced much farther. The sun turns on its axis: so does the star Algol: so do the stars called  $\beta$  Lyræ,  $\delta$  Cephei,  $\eta$  Antinoi,  $\sigma$  Ceti, and many more; most probably all. From what other cause can we so probably account for their periodical changes? Again, our sun has spots on its surface: so has the star Algol, and so have the stars already named, and probably every star in the heavens. On our sun these spots are changeable: so they are on the star  $\sigma$  Ceti, as evidently appears from the irregularity of its changeable lustre, which is often broken in upon by accidental changes while the general period continues unaltered. The same little deviations have been observed in other periodical stars, and ought to be ascribed

ascribed to the same cause. But if stars are suns, and suns are inhabitable, we see at once what an extensive field for animation opens itself to our view.

It is true that analogy may induce us to conclude, that, since stars appear to be suns, and suns, according to the common opinion, are bodies that serve to enlighten, warm, and sustain a system of planets, we may have an idea of numberless globes that serve for the habitation of living creatures. But if these suns themselves are primary planets, we may see some thousands of them with our own eyes, and millions by the help of telescopes, when at the same time the same analogical reasoning still remains in full force with regard to the planets which these suns may support.

In this place I may, however, take notice, that, from other considerations, the idea of suns or stars being *merely* the supporters of systems of planets, is not absolutely to be admitted as a general one. Among the great number of very compressed clusters of stars I have given in my catalogues, there are some which open a different view of the heavens to us. The stars in them are so very close together, that, notwithstanding the great distance at which we may suppose the cluster itself to be, it will hardly be possible to assign any sufficient mutual distance to the stars composing the cluster, to leave room for crowding in those planets, for whose support these stars have been, or might be, supposed to exist. It should seem, therefore, highly probable that they exist for themselves; and are, in fact, only very capital, *lucid*, primary planets, connected together in one great system of mutual support.

As in this argument I do not proceed upon conjectures, but have actual observations in view, I shall mention an instance in the clusters No. 26, 28, and 35, VI. class, of my catalogue of nebulae, and clusters of stars. (See Phil. Transf. Vol. LXXIX. Part II. p. 251.) The stars in them are so crowded that I cannot conjecture them to be at a greater apparent distance from each other than five seconds, even after a proper allowance for such stars, as, on a supposition of a globular form of the cluster, will interfere with one another, has been made. Now, if we would leave as much room be-

tween



tween each of these stars as there is between the Sun and Sirius, we must place these clusters 42104 times as far from us as that star is from the sun. But, in order to bring down the lustre of Sirius to that of an equal star placed at such a distance, I ought to reduce the aperture of my 20-foot telescope to less than the two-and-twenty hundredth part of an inch; when certainly I could no longer expect to see any star at all.

The same remark may be made with regard to the number of very close double stars, whose apparent diameters being alike, and not very small, do not indicate any very great mutual distance: from which, however, must be deducted all those where the different distances may be compensated by the real difference in their respective magnitudes.

To what has been said may be added, that, in some parts of the milky way, where yet the stars are not very small, they are so crowded, that in the year 1792, Aug. 22, I found by the gages that, in 41 minutes of time, no less than 258 thousand of them had passed through the field of view of my telescope\*.

It seems, therefore, upon the whole, not improbable that, in many cases, stars are united in such close systems as not to leave much room for the orbits of planets or comets; and that consequently, upon this account also, many stars, unless

\* The star-gages ran thus:

From  $19^{\text{h}} 35'$  to  $19^{\text{h}} 51'$  600 stars in the field

19 51 — 19 57 440

19 57 — 20 12 360

20 12 — 20 16 260

The breadth of the sweep was  $2^{\circ} 35'$ , the diameter of the field  $15'$ , and the mean polar distance  $73^{\circ} 54'$ . Then let

F, be the diameter of the field of view,

S, the number of stars in each field,

B, the breadth of the sweep, plus F,

T, the length of the sweep, expressed in minutes of space,

$\phi$  the sine of the mean polar distance,

C, the constant fraction .7854,

and the stars in these four successive short sweeps will be found by the

expression  $\frac{BTS\phi}{F^2C}$  equal to 133095.36601. 74866. 14419. or in all 258981.

we would make them mere uselefs brilliant points, may themselves be lucid planets, perhaps unattended by satellites.

#### POSTSCRIPT.

The following observations, which were made with an improved apparatus, and under the most favourable circumstances, should be added to those which have been given. They are decisive with regard to one of the conditions of the lucid matter of the sun.

*Nov. 26, 1794.* Eight spots in the sun, and several subdivisions of them, are all equally depressed.

The sun is mottled every where.

The mottled appearance of the sun is owing to an inequality in the level of the surface.

The sun is equally mottled at its poles and at its equator; but the mottled appearances may be seen better about the middle of the disc than towards the circumference, on account of the sun's spherical form.

The unevenness arising from the elevation and depression of the mottled appearance on the surface of the sun, seems in many places to amount to as much, or to nearly as much, as the depression of the penumbrae of the spots below the upper part of the shining substance, without including faculae, which are protuberant.

The lucid substance of the sun is neither a liquid nor an elastic fluid; as is evident, from its not instantly filling up the cavities of the spots, and of the unevenness of the mottled parts. It exists, therefore, in the manner of lucid clouds swimming in the transparent atmosphere of the sun; or rather, of luminous decompositions taking place within that atmosphere.

IV. *Extract of a Memoir, and Experiments on the Nutrition of Plants.* By M. RAFFN, *Affessor in the Office of Commerce at Copenhagen* \*.

**H**ASSENFRATZ considers carbon as the substance which nourishes vegetables †. Ingenhous, in his work on the nutrition of plants, published in 1797, endeavours to prove, that if carbon has any influence in this respect, it can be only in the state of carbonic acid, as that acid is absorbed and decomposed ‡ by vegetables; while the ligneous carbon, furnished by Nature, produces no effect on the expansion of plants. Mr. A. Young has endeavoured to demonstrate the same thing by experiments. M. Raffn, desirous of discovering the truth amidst these contradictory opinions, made, for three years, a series of experiments, from which he concludes, by the expansion, size, and colour of the plants employed, that carbon, either vegetable or animal, has a decided influence in the nourishment of vegetables. What is new, and particularly worthy of remark in these researches, is, that, according to M. Raffn, the carbonic acid produces exactly the same effect as charcoal of wood. The following are the experiments which conducted the author to this result:—

Having half filled a large box with brick-kiln rubbish, or pounded tiles, which he covered with a layer of vegetable earth, he placed over the latter a stratum of carbonate of lime (pounded limestone) and alum, and then two or three of vegetable mould, in which he sowed barley. He presumed that the sulphuric acid of the alum, abandoning the argil to join the lime, with which this acid has greater affi-

\* Translated from the Danish, with notes by C. Vauquelin and Brogniart.

† This opinion of C. Hassenfratz appears to be very probable; but, as he says, the carbon must be held in solution by hydrogen gas, by water, by that saponaceous extract which separates from vegetables when in a state of putrefaction, or by any other liquid.

‡ This decomposition is possible, but it has not yet been proved by any direct experiment.

nity, the carbonic acid gas would be disengaged, which would furnish the means of knowing its influence on the vegetation. Another box was filled merely with mould, a third merely with charcoal, and a fourth with animal carbon. These were to be employed in comparative experiments, and barley was sown in them all.

Though the plants which germinated in the first box were sown in a stratum of mould about two or three inches in thickness, they had no resemblance, either in strength or colour, to those sown in the second box filled with mould alone; but they had, on the other hand, such a perfect resemblance to those of the third box filled with charcoal, that it would have been difficult to distinguish the difference. This resemblance continued several weeks, after which they seemed to have not quite the same vigour as those which grew in the charcoal, for which it is not difficult to assign a reason. The author convinced himself that a decomposition had really taken place, because, on examining the first box in autumn, he found that sulphat of lime had been formed. These experiments seem proper to conduct to a knowledge of the manner in which plants attract the carbonic principle, which all the researches of the author demonstrate to be necessary for vigorous vegetation. He proposes to repeat them on a larger scale, and to vary them as much as possible\*. He repeated, several times, those of M. Humboldt on germination, accelerated by the oxygenated muriatic acid, and always with success, though with this difference, that this acid did not favour vegetation so much as that philosopher asserted.

M. Raun sowed barley in a mixture of mould, sand, and manganese, in order to see whether the oxygen gas would

\* These experiments would be more conclusive had not the author added mould in the boxes into which he put the rubbish. It is well known that mould contains a great quantity of carbon, exactly in the state which renders it fit for the nutrition of vegetables.

Mr. A. Young, on the other hand, asserts, that plants grow exceedingly ill in charcoal: and this observation agrees more with the others, and with the reasoning, which induces us to believe that carbon must be dissolved to enter into combination with the other principles of vegetables. As plants grow exceedingly well in pure water till a certain period, it would appear that they ought to grow equally well in watered charcoal.

not

not be disengaged in such a manner as to produce some effect on plants. At first he obtained no effect; but having watered this box with diluted sulphuric acid, he remarked that the barley visibly grew faster in this box than in those not watered in the same manner\*.

Of all the mixtures which he tried for sowing, none appeared to him better than that of equal parts of charcoal, mould, and sand, moistened with water filled with infusion animals, which may be easily obtained by steeping flax in the water destined for that purpose. He observes, on this occasion, that, of all the substances he tried, flax is that which furnishes the most of these animalculæ. An incredible multitude of them are found in the water in which women dip their fingers when they are employed in spinning. The water put into a vessel for that use in the morning, is found filled with them in the evening. The author ascribes to these small animals a much greater influence on vegetation than has hitherto been believed.

Hassenfratz relates, that he could not make plants vegetate well in simple earths. The author asserts, that he had great success when he reared them in pure silex, quartz, sand thrice washed, fine sand from the sea-shore, &c. But these plants continued stunted and pale, and their roots were twice as long as the whole of the part above the earth. In charcoal, on the other hand, the parts were large and vigorous; they were of an exceedingly dark colour, and their roots were not a sixth part of the length of the plant itself†.

Coal-ashes, on which the German and English farmers bestow such praise, destroy the plants if the soil contains an eighth part of that admixture. The leaves become faded, as

\* The sulphuric acid cold does not disengage the oxygen of the oxyd of manganese: besides, according to the experiments of Ingenhous, this acid alone, in small quantity, seems to have the property of rendering vegetation more active.

† The first results are perfectly similar to those obtained by C. Hassenfratz. In regard to the second, they depend on the purity of the charcoal employed, which may contain wood undecomposed, and consequently disposed to putrify, and to yield a liquid which may hold the carbon in solution.

if scorched, at the end of from fifteen to twenty days, and the plants themselves die at the end of four or five weeks.

No seed germinates in oil. A single grain of common salt in two hundred grains of water is sufficient to retard the vegetation of plants, and may even kill them if they are watered with that saline liquor\*.

Shavings of horn, next to infusion animals, are the most favourable to vegetation: charcoal holds the third rank.

*V. On the Assaying of Iron Ores and Iron-Stones by Fusion.*  
By Mr. DAVID MUSHET, of the Clyde Iron-Works†.

IT will easily be conceived, from the mode of operations which I have adopted, that, in order to procure accurate results, the proportion of flux must be varied according to the mixtures in the iron-stones or ores; and that no universal solvent can be used as capable of assaying *all* ores.

As the gradation of mixtures in the ores is almost imperceptible, there are, in fact, no fixed limits by which Nature has distinguished the various classes: we find all the varieties diminishing their predominant earth, and assuming, in equal proportions, those of each other, thus constituting the class of equalised mixtures; yet, here, the variety of combination ceases not, the predominating earth gradually becomes the minor part of the mixture, and that which only held a second rank, as to quantity, is now the chief component earth; the permutation goes round, till the earth, which existed in the most sparing quantity, now predominates to excess.

\* C. Sylvestre obtained a result absolutely similar, by employing marine salt as manure.

† The present is a part of the communication from Mr. Mushet which appeared in our Number for July last, (Vol. IV. p. 178,) but by an oversight of the Editor was omitted in its place. It contains the table of proportions alluded to in our Number for September last (Vol. IV. p. 380.) requisite for the obtaining from all the various iron-stones an accurate assay, and should have immediately followed Mr. Mushet's article given in our July number.

In such an infinity of variation it is difficult to arrange the combinations of which these substances are capable. To derive the name of a class, or genus, from the predominancy of an earth, seems most eligible; and to consider those as varieties of the same class, which are altered by the proportion of the second and third mixtures. Again, each of these varieties are susceptible of a multiplicity of modifications before an earth is so far diminished as to give an ascendancy to another, or before the third rank of proportion has assumed that of the second or first. The simple combination of the earths, and their degrees of predominancy, may be thus arranged:—

	1st Variety,		2d Variety.	
Argillaceous iron-stone	{ Iron	-	-	Iron
	{ Clay	-	-	Clay
	{ Lime	-	-	Silex
	{ Silex	-	-	Lime
Calcareous iron-stone	{ Iron	-	-	Iron
	{ Lime	-	-	Lime
	{ Clay	-	-	Silex
	{ Silex	-	-	Clay
Siliceous iron-stone	{ Iron	-	-	Iron
	{ Silex	-	-	Silex
	{ Lime	-	-	Clay
	{ Clay	-	-	Lime

As these become varied, they form the class of equalised mixture.

To assay any of these varieties, a flux peculiar to the nature of the mixture is necessary; so that the changes of proportion in the solvent ought to extend to seven, including the class of equalised mixtures, in order that the precise same quality of crude iron may be produced from all the varieties of iron-stone. The modification of each variety will be found to be sufficiently accurate, if assayed by the flux peculiar to itself. The arrangement of the three classes of ores into two varieties, each forming a distinct stage of combination, indicated by the predominancy of the first and second earth, are, with the neutral class, sufficiently minute for any purpose in the assay-furnace, and are sufficient to form an accurate

accurate and extensive knowledge of the analogy of these results with those in the blast-furnace.

### TABLE of PROPORTIONS of FLUXES.

Let the earthy part of an argillaceous ore be composed of clay 9, lime 6, sand 3 = 18.

To assay 4 troy ounces of this ore	-	or	1920 grains,
add 4	_____	bottle glafs	— 1920
3	_____	chalk	— 1440
$0\frac{1}{2}$	_____	charcoal	— 240
<hr/>			<hr/>
$11\frac{1}{2}$			5520

Let the second variety of argillaceous ores contain, clay 10, filix 7, lime 3 = 20.

In this case, 4 ounces troy of ore	-	or	1920 grains,
would require 4	_____	bottle glafs	— 1920
4	_____	chalk	— 1920
$0\frac{3}{4}$	_____	charcoal	— 360
<hr/>			<hr/>
$12\frac{3}{4}$			6120

Let the first variety of the calcareous genus of iron-stone be supposed to contain, of earthy mixtures, lime 14, clay 6, filix 4 = 24.

When this iron-stone is to be assayed,

to 4 ounces troy	-	-	or	1920 grains,
add 5	_____	bottle glafs	-	— 2400
$1\frac{1}{2}$	_____	chalk	—	— 720
$0\frac{3}{4}$	_____	charcoal	—	— 360
<hr/>				<hr/>
$11\frac{1}{4}$				5400

Again, let the second variety of the calcareous genus be supposed to contain, lime 10, sand 6, clay 4 = 20.

I would add to 4 troy ounces	-	or	1920 grains,
4	_____	bottle glafs	— 1920
2	_____	chalk	— 960
$0\frac{1}{2}$	_____	charcoal	— 240
<hr/>			<hr/>
$10\frac{1}{2}$			5040

Let



Let the first variety of filiceous ores be supposed to contain, filex 12, clay 8, lime 5 = 25.

For an assay of 4 troy ozs. of ore	-	or 1920 grains,
add 4	————	chalk — 1920
3	————	bottle glafs — 1440
$o\frac{3}{4}$	————	charcoal — 360
<hr/>		<hr/>
11 $\frac{3}{4}$		5640

And, lastly, let the second variety of this genus of iron-stone be supposed to contain, filex 10, lime 7, clay 5 = 22.

To 4 troy ounces of the ore	-	or 1920 grains,
add $3\frac{1}{2}$	————	chalk — 1680
3	————	bottle glafs — 1440
$o\frac{3}{4}$	————	charcoal — 360
<hr/>		<hr/>
11 $\frac{1}{4}$		5400

Clafs of equalised mixtures, composed of, clay 7, lime 7, filex 7 = 21.

To assay of this ore 4 troy ounces	-	or 1920 grains,
add $3\frac{1}{2}$	————	bottle glafs — 1680
$2\frac{1}{2}$	————	chalk — 1200
$o\frac{1}{2}$	————	charcoal — 240
<hr/>		<hr/>
10 $\frac{1}{2}$		5040

VI. On the real Origin of that Resin known under the Name of Sandarac, and that of Gum Arabic. By M. SCHOUSBOE\*.

**G**UM Sandarac is an article of trade brought from the southern provinces of the kingdom of Morocco. About six or seven hundred quintals of it are exported every year from Santa Cruz, Mogador, and Saffy. In the language of the country it is called *El graffa*. The tree which produces it is a *Thuia*, found also by M. Vahl in the kingdom of

\* From a Danish Journal, entitled, *The Physical, Medical, and Economical Library*, Part III. 1759.

Tunis\*. It was made known several years ago by Dr. Shaw, who named it, *Cypressus fructu quadrivalvi, Equiseti instar articulatis*; but neither of these learned men was acquainted with the economical use of this tree; probably because, being not common in the northern part of Barbary, the inhabitants find little advantage in collecting the resin which exudes from it. This resin hitherto has been ascribed to the *Juniperus communis*, *Juniperus Lycia*, or the Cedar of Lebanon, without reflecting that the *Juniperus communis* does not grow in Africa, and Sandarac seems to belong exclusively to that part of the world. M. Schoufboe, who saw the species of *Thuia* in question, says that it does not rise to more than the height of twenty or thirty feet at most, and that the diameter of its trunk does not exceed ten or twelve inches. It distinguishes itself, on the first view, from the two other species of the same genus, cultivated in gardens, by having a very distinct trunk, and the figure of a real tree; whereas in the latter the branches rise from the root, which gives them the appearance rather of bushes. Its branches also are more articulated and brittle. Its flowers, which are not very apparent, shew themselves in April; and the fruit, which are of a spherical form, ripen in September. When a branch of this tree is held to the light, it appears to be interspersed with a multitude of transparent vesicles which contain the resin. When these vesicles burst in the summer months, a resinous juice exudes from the trunk and branches, as is the case in other coniferous trees. This resin is the Sandarac, which is collected by the inhabitants of the country, and carried to the ports, from which it is transported to Europe. It is employed in making some kinds of sealing-wax, and in different sorts of varnish. In 1793 a hundred weight of it cost in Morocco from 13 to 13½ piastres, which make about 3*l.* 5*s.* to 3*l.* 7*s.* and 6*d.* sterling. The duty on exportation was about 7*s.* 6*d.* sterling *per* quintal.

Sandarac, to be good, must be of a bright-yellow colour, pure and transparent. It is an article very difficult to be

\* A complete description of it, with a good figure, may be found in his work entitled, *Symbol. Botan.* Part II. p. 96. Plate XLVIII. under the name *Thuia articulata*.

adulterated. Care, however, must be taken that the Moors do not mix with it too much sand. It is probable that a tree of the same kind produces the gum fandarac of Senegal, which is exported in pretty considerable quantities.

Another article of commerce in which the kingdom of Morocco participates with Senegal, is that gum called Gum Arabic, known by the name of *Al leilk*. The tree which produces it grows only in the southern provinces of that state. The quantity of this article exported to the different parts of Europe from the ports of Morocco may amount to about eight or nine quintals. M. Schoufboe says, that this tree is the *Mimosa nilotica*, named in the country *Al thlab*; but this is no reason why the same kind of gum should not be collected in the more southern countries of Africa from the *Mimosa Senegal*, and even from other trees of the same species, as we are told by various authors. In Barbary the people make a difference between the gum of Senegal and that of the country. The former is preferred on account of its purity, whiteness, and transparency, which in general are the properties sought for in this article.

The gum which I collected myself in the province of Mogador, says M. Schoufboe, exudes from the trunk and branches of the tree, in the same manner as that of our fruit-trees. It is in round lumps, of the size of a hazle-nut, or rather that of a walnut. These lumps, indeed, by becoming united to each other, form masses sometimes of the size of the fist, or even of the head; but this only happens in consequence of the adhesion which takes place between the pieces of gum, when still fresh, after they have been detached from the tree, and chiefly at that part which was attached to the bark, where the resinous juice has not had time to harden. If earth, small stones, and other foreign bodies, are sometimes found in these masses, it arises from fraud; and M. Schoufboe suspects that this circumstance has given rise to the opinion of the gum being found at the bottom of the tree, and that it exudes from the roots, which, as he thinks, is void of foundation. Were this the case, it appears to him that, besides the earth and sand with which the gum is accidentally dirtied, balls of these matters ought to be found

in the inside, and even so mixed with the mucilaginous substance that it would be impossible ever to purify it completely; while, on the other hand, the gum which comes from Senegal is still purer than that of Barbary.

M. Schousboe, however, observes, that the sandarac and gum exported from the port of Saffy have a brown or reddish colour; but he ascribes this colour to the quantity of the red oxyd of iron mixed with the soil of the province of Abda, where this port is situated. This oxyd communicates its colour even to the whitest wool; and the inhabitants of that province may be distinguished by the reddish tint of their clothes, which cannot be entirely destroyed by any process. In the months of July and August, when heavy dews fall, the gum loses a great deal of its transparency, as well as of its other good qualities. A hundred weight of this substance cost at Mogador, in 1793, about 2*l.* sterling, without including about 4*s.* custom-house duty. The gum does not appear to be employed by the inhabitants of Morocco for any purpose whatever: the whole of what they collect is sold to the different commercial nations of Europe.

VII. *On the Pestilential Diseases which, at different times, appeared in the Athenian, Carthagian and Roman Armies, in the Neighbourhood of Syracuse. By the late E. H. SMITH, Physician\*.*

#### SECTION I.

**S**YRACUSE, the most beautiful of all the cities built by the Greeks, was founded by Archias, a Corinthian, of the race of Hercules\*. He first expelled the natives from Ortygia, where he commenced the city, which was afterwards extended to the neighbouring continent. This place, so celebrated in ancient history, the birth-place of Archimedes, and theatre of many memorable transactions, now reduced to a miserable town, of inferior consequence even in Sicily, is situated in north latitude 37° 5', a little above Cape Passara

\* From the *American Medical Repository*, Vol. II. No. 4.

† Cicero in Ver. Act. II. Lib. iv. § 117. Thucydides, B. vi.

(the

(the ancient Pachynum), the south-western extremity of the island of Sicily.

The climate of Sicily, in general, is represented as favourable to health \* ; and, notwithstanding its insular situation, more analogous, in the qualities of temperature and humidity, to the hilly, than to the Atlantic divisions of South-Carolina and Georgia. The winter<sup>e</sup> is remarkably mild : with the exception of a few days, it equals the finest spring weather in the North of Europe †, and the Eastern States of America ; and the shade is found pleasant, in the middle of the day, even in the month of January ‡. At Syracuse, in particular, the season is so little affected by the severities of cold and tempest, that, during its whole course, and in its most boisterous state, the sky is never totally obscured for a single day §. Yet in this climate did Verres, the voluptuous prætor of Sicily, that monster of atrocity and lasciviousness, so entirely seclude himself from all impact and influence of the atmosphere, as only to learn the approach of spring by observing the dew glisten on the verdure which surrounded his palace ||. As the season advances, the heat rapidly increases, till, in the summer, it is no longer to be endured by strangers, and exertion, as well as enjoyment, is temporarily suspended. In autumn, the frequent rains, which are common throughout this season, and the heats of the middle of the day, contrasted with the extreme chilliness of the evenings, render it less pleasant and salubrious than any other part of the year ¶. It is to the excessive sultriness of the summers that a late sensible writer \*\* ascribes the imperfection of many,

\* Benige Berichten, &c. *i. e.* Some account of the Prussian, Austrian, and Sicilian monarchies, &c. See Monthly Review Enlarged, Vol. XIV. p. 491.

† Swinburne's Travels in the Two Sicilies, Vol. II. § 49. *Dubl. edit.*

‡ Keysser's Travels, Vol. III. p. 33.—Translation.

§ Cicero in Ver. Act. II. Lib. v. § 26.

|| Cicero in Ver. Act. II. Lib. v. § 12.

¶ Travels in Sicily and Malta, by M. De Non, p. 307, 333, and 383.—Translation.

\*\* Benige Berichten, &c.—“ The soil is exceedingly fertile ; but, from the great heat of the climate, many of our most nourishing and refreshing vegetables

many, and the total want of others, of the most nutritious and refreshing vegetables of the northern climates, in despite of that fertility of soil which, from time immemorial, has conferred on Sicily the appellation of the granary of Europe.

But, notwithstanding the general pleasantness and healthfulness of this island, the concurrent testimony of ancient and modern writers evinces the noxious condition of particular places. "The least stagnant water is sufficient, in the heats of summer, to poison the atmosphere: its effects on the countenances of the poor people, who live in its vicinity, are very evident; and a stranger, who travels through the island in this season, ought to avoid ever passing a night near them \*." As soon as the sun enters the Lion, this country becomes the house of death: fevers, of the most malignant kind, seize upon the imprudent or unfortunate wretch that spends a night near them (ponds and marshes); and few escape with life, when attacked by so virulent a disorder †." Instructed, probably, by experience of the calamities consequent on a near residence to marshes and stagnant waters, the inhabitants in various parts of Sicily have built their towns on adjoining eminences ‡. To avoid the dangers which beset the stranger in journeying through this country in the sultry and autumnal seasons, was probably one of the motives of the Roman prætors for performing the tour of the island in the time of harvest §. By a neglect of similar prudence, the celebrated M. De Non became affected with a violent fever and ague, after an incautious exposure to the heat of the sun and the chill of the evening, in an unwholesome part of the country, and in the month of September ||.

From this brief account of the climate of Sicily, it will be evident, that, how favourable soever it may be esteemed, in general, to the health and longevity of the natives, and how vegetables will not thrive in it. Currants, raspberries, and gooseberries, are unknown to the natives; and foreigners, who have attempted to cultivate them, have never been able to succeed."

\* Eenige Berichten, &c.

† Swinburne's Travels, Vol. II. § 49. Dublin edit.

‡ Swinburne's Travels, Vol. II. p. 300. Dublin edit.

§ Cicero in Ver. Act. II. Lib. v. § 80.

|| De Non's Travels, p. 383.—Translation.

pleasant soever many parts of it deserve to be considered as a winter and even vernal residence for strangers; yet, in other seasons, and particularly for visitors from colder countries, it must be in most parts unsalutary, in others certainly fatal, and, in some, not to be long continued in, with impunity, by the inhabitants themselves. And, as this remark refers wholly to ordinary years and circumstances, it will afford some ground for estimating the effects of one of the most unhealthy situations in the country, in the sickly season of the year, with a constitution of the atmosphere favourable to epidemic diseases, on an army of foreigners, tumultuous and ill-accommodated, worn down with fatigue, or sinking in despondency.—With this reflection impressed on the mind, we may now proceed to a description of the city of Syracuse, and the country immediately adjacent, as they existed in ancient times, occasionally supplying illustrations from the accounts of modern travellers.

The once beautiful city of Syracuse consisted of five principal divisions, traces of all which are still discernible. 1. The isle Ortygia, Nafos, or Nafon: 2. Achradina: 3. Tyche, or Tycha: 4. Neapolis, or the new city: and, 5. Epipolæ\*.

*Ortygia*, enclosed by the two ports—the great port on the west and the little port on the east, and connected with the continent by a bridge—is of an oblong form, and about two miles in circumference†. Here the original settlement commenced, and this was always considered as the wealthiest and most desirable part of Syracuse. It was the ancient residence of the kings; and, in the time of Cicero, still contained the house of Hiero. It was ornamented with various public buildings, particularly the temples of Diana and Minerva; and, in part, watered by the poetic fountain of Arethusa. As the population increased, the sound, which divided Ortygia from the Continent, was filled up, and the isle converted into a peninsula‡. Carlos III. of Spain removed the earth by which they were united; and Ortygia is again an island, and connected with the Continent, as formerly, by a bridge§.

\* Cicero in Ver. Act. II. Lib. iv. § 118. De Non, p. 304. et sequent. Swinburne, Vol. II. p. 309.

† De Non. Swinburne.

‡ De Non. Swinburne.

§ Brydone's Tour, Letter xiii.

The situation of Ortygia is important, for it commands both the ports; and though supposed itself to be commanded by Achradina, yet, while that quarter of the city remained in the possession of the Syracusans, together with the isle, and the opposite promontory of Plemmyrium, the town was nearly inaccessible to a naval force\*. At present Ortygia is the only city. To this state it was reduced by the Mussulmen. It is strongly defended towards the land, weakly towards the sea-side. Its quay is small; its streets narrow, winding, and wretchedly built; and its population does not exceed 18,000 persons†. The fountain of Arethusa, after repeated changes of situation‡, is still discoverable in the west part of the isle; but its beauty and its honours have fled with the mythology to which it is indebted for its fame.

*Achradina.* The quarter of Achradina, at the period to which this essay has particular reference, was the most spacious, well built, and strongly fortified part of the city. It extends over two considerable levels, divided by a natural wall of calcareous rocks; the one as elevated as Tyche; the other, and more considerable, on a plane with Ortygia, and thence conferring on this quarter the character of the lowest division of Syracuse. The eastern part was the most commodious, and not less extensive than the modern Paris. The whole was adorned with a large forum, a beautiful portico, prytaneum, curia, and the temple of Jupiter Olympus; bordered on three sides by the sea, by the great port on the west, the lesser on the south, and the port of Trogilus on the east; and, on all sides, nearly impregnable. The rocks of this quarter of Syracuse, which are formed by marine depositions, possess the singular property of dissipating or absorbing the moisture of dead bodies so speedily, that they are preserved in vaults excavated for the purpose, in their proper form and habiliments. Achradina is remarkably fertile, and naturally adapted for bringing to perfection every tropical production§.

\* De Non. Swinburne.

† Swinburne, Vol. II. p. 311, 312. De Non, p. 304.

‡ De Non, p. 307.

§ Cicero ut antea. De Non, p. 321, et sequent. Swinburne, Vol. I. p. 313, 314, 315. Rollin's Rom. Hist. Vol. V. p. 204.



*Tyche*, or the third city, extended northerly from Achradina to Epipolæ; commencing at the bottom of the port of Trogilus, which forms its south-eastern boundary. East, it was defended by a strong wall; and a wall divided it, on the west, from Neapolis. The famous gate of Hexapylon was on the eastern wall, and opposite to the little town of Leon. Tyche was ornamented by the Gymnasium; and, though now desolate, was once filled with inhabitants. It is very elevated, rising in rapid gradation from the wall of Achradina\*.

*Neapolis* ran nearly parallel, and in the same direction with Tyche; terminating on the north, with that quarter, at Epipolæ. On the east, south, and west, it was equally protected by a wall. The upper and northern part of Neapolis, as of Tyche, was elevated; its south-western extremity was considerably lower. A noble theatre, and the temples of Ceres and Bacchus, were its most distinguished ornaments†.

*Epipolæ* was originally a piece of high ground without the city, and afterwards so little inhabited that it is not mentioned by Cicero in his description of Syracuse. The most elevated situation, and commanding Tyche and Neapolis, it was judiciously inclosed by Dionysius I. who surrounded it with a wall of near four miles in extent. Its additional defence was the fortress of Labdalon, at its bottom, on the east; and that of Euryalus, at its top, on the north‡.

The whole extent of the city of Syracuse, according to Strabo, whose account is verified by the concurrent testimony of Mr. Swinburne, was equal to twenty-two miles and four furlongs English measure§.

To this brief description of the principal divisions of this famous city, it is necessary to add a few remarks relative to its immediate vicinage, and the waters by which it is surrounded.

The *Great Harbour*, or Port, is about five miles in circumference, and forms, at its north-western extremity, the

\* Cicero. Rollin. De Non. Swinburne.

† Cicero. Rollin. De Non. Swinburne.

‡ Rollin, Swinburne. De Non.

§ Swinburne, Vol. II. p. 309.

Gulf of Dascon. Lower down, and opposite to Ortygia, it is contracted by the promontory and fort of Plemmyrium. The *Little Port*, or *Portus Marmoreus*, divides Ortygia from the Continent on the east, and washes the southern wall of Achradina. The *Bay of Thapsus* runs up behind Achradina on the east, and forms, by the junction of its western extremity with the city, the *Port of Trogilus* \*.

At the distance of about two miles from Ortygia, and somewhat less than a mile and a half from Neapolis, (whose western wall crossed the low grounds through which it runs,) the river Anapus empties into the great harbour. This stream, which is only twenty-four feet wide, and twelve or fifteen deep at its mouth, flows, in a serpentine course, through a small extent of country, which, though slightly elevated on its south or south-western side, on the north and north-west consists of an extensive marshy plain †.

Between the Anapus and the promontory of Plemmyrium was situated the little suburb of Olympia, surrounding the site of the ancient temple of Jupiter Olympus, built upon an eminence, and bounded on either side by the vast Lyfimelian marshes, extending from the head of the great harbour, half covered with water in the vernal months, and exhaling, under a vertical sun, the most unwholesome and pernicious vapours ‡.

The preceding detail, it is hoped, will neither be found altogether tiresome, nor foreign to the purpose of this essay. Some part of it might, indeed, have been omitted, as not directly essential to the main design of the paper; but there would have been less unity in the description, and some portion of the subsequent narration might have been less perfectly comprehended,

\* Rollin. Swinburne. De Non.

† Rollin. Swinburne. De Non. Thucydides, B. vi. and vii.

‡ Rollin. De Non. Swinburne. Watkin's Travels, Letter xxiv.

The reader will find great assistance in comprehending distinctly every part of the preceding description, from consulting the plan of Syracuse in Rollin's Ancient History, or that in his Roman History, which, though inaccurate in some respects, I have reason to believe the most correct.

SECT.

SECT. II.—The pestilential diseases prevailing in the neighbourhood of Syracuse, to which the present inquiry is limited, occurred at three distinct periods: at the three sieges of that city, by the Athenians and their allies, under Nicias; by the Carthaginians, under Imilcon; and by the Romans, under Marcellus. It is exceedingly to be regretted, that, considering the singular mortality which distinguished each of these plagues, the information concerning them is so scanty and general. But the precision which remarkably characterises that which remains, renders it of sufficient importance to deserve particular attention. It is the purpose of the present Section to place before the reader, in a succinct narration, the circumstances which the historians have preserved relative to each of these interesting events.

In the second year of the famous contest between the Syracusans and the Athenians, of which Thucydides has left so minute and so touching an account, the Athenians, whose principal encampments were on the east of Syracuse—at Leon, at Thapfus, at Labdalium, and on the port of Trogilus—undertook the construction of a wall, which extended to Epipolæ, and was designed, after crossing the plain and the marshes of the Anapus, to reach to the great harbour on the west, and inclose the city. From the completion of this enterprize they were prevented by the exertions of the Syracusans, who carried a division-wall from the city across the marsh, and thus opposed an insurmountable barrier to the progress of their besiegers. To promote his design, of the success of which he now began to entertain doubts, and to enable him more effectually to annoy the enemy by water, the Athenian general fortified and transported his army to Plemmyrium. From this time the fortune of the Athenians changed: their fatigue was great and incessant; every trifling success was succeeded by some sad reverse; relief was distant and uncertain; and they became a prey to despondency, not a little heightened by a sense of the injustice of their cause, their exemplary humiliation, and the unexpected superiority of their antagonists. Under these circumstances, in the autumnal season, and “encamped on marshy and

unwholesome ground \*," a situation "always unhealthy for an army, and especially in this season of the year †," a pestilential disease shewed itself in the camp, extending with the progress of the season, and the continued exposure of the soldiery, or, as Plutarch expresses himself, by contagion; till, harassed by the enemy on one part, and worn down by fatigue and sickness on the other, the Athenians were driven to the sad necessity of attempting a forced and secret retreat, with the dereliction of their camp, their wounded, and their sick. The consummate eloquence of Thucydides alone is adequate to the description of this scene of horrors.

Of the numbers who perished by this pestilence, and of its particular symptoms, no record is transmitted down to us. The event of this siege is known. The Athenians were surrounded on their retreat, and defeated with immense destruction, and under the most melancholy circumstances of distress: their general, worthy of a better fate, was cruelly put to death; and the greater part of those who were made prisoners perished in Syracuse, the victims of diseases induced by excessive labour and unwholesome food. A few (in such honour was poetry among the ancients,) were emancipated by the recollection and recitation of even a single verse of the pathetic tragedies of Euripides ‡.

At a subsequent period, and during the war between the Carthaginians and Dionysius the elder, Himilco, or Imilcon, after several successful enterprises in other parts of Sicily, marched against Syracuse. He invaded it with an army of 300,000 foot and 3000 horse; while the Carthaginian fleet, of 200 ships, under the command of Mago, followed by 500 barks, entered the great port in a triumphal manner, and laden with the spoils of the ravaged cities of the island. Imilcon pitched his tent in the very temple of Jupiter, then standing at Olympia; and his army encamped around him.

\* Smith's Thucydides, Vol. II. p. 236.

† Plutarch, art. Nicias.

‡ *Ibid.*—For the particular history of the siege of Syracuse, the reader is referred, generally, to Thucydides, B. vi. and vii.; and to the Life of Nicias, by Plutarch.

In this situation, "an eminence between two morasses, highly favourable for a camp, and for rendering it impregnable," he continued thirty days, laying waste the country on all sides, plundering the temples, and demolishing the tombs of the kings, and in vain offering battle to the Syracusans, who had not the courage to attack or oppose him. Nature, more powerful than themselves, arrested the progress of that fate which threatened to overwhelm them. "It was now in the midst of summer, and the heat this year was excessive." While Imilcon continued at Olympia, spreading devastation around him, and anticipated a signal revenge upon the hostile Syracusans, a pestilential malady shewed itself in his camp. It appeared first among his auxiliaries, the Africans, who, forced into the service by fear of the Carthaginians, whom they hated, were probably least carefully accommodated. From them it soon extended, by the increasing virulence and activity of the exciting cause, throughout the army. Neither care nor medicine afforded relief. At first the sick received some assistance from the well; but the pestilence and the mortality multiplied themselves so rapidly in every direction that this soon became impossible, nor were there men to be found for the performance of the rites of sepulture to the accumulating dead. "Violent dysenteries, raging fevers, burning entrails, and acute pains in every part of the body," were the usual symptoms of this terrible disease. Some were even seized with madness, and in their phrensy attacked and endeavoured to destroy all that were exposed to their assaults. Meantime the Syracusans, in health and safety in their elevated, dry, and airy city, watched the progress of the pestilence among their enemies, and, profiting by their forlorn condition, captured or involved in flames their mighty fleet; and, storming their camp, impregnable to the greatest force when defended by even a small body of healthy troops, defeated them with exemplary destruction. Thus beset, enfeebled by disease and humbled by defeat, the proud Imilcon, who, a short time before, held the conquest, not only of Syracuse, but of Sicily, too cheap and easy a victory for so formidable an host, was reduced to purchase his safety

for 300 talents, and fled, leaving unburied the carcases of 150,000 of his soldiery \*.

The history of the last of those pestilential diseases, which are particularly selected for present consideration, deserves a longer and more attentive examination. Furnishing excellent illustration of several important points in the history of these disorders, the reader will doubtless excuse such preliminary narration as may conduce to place it more fully before him.

The Roman army, under the command of Marcellus, decamped from their station at Leontium, the modern Lentini, and, arriving at Syracuse, encamped at Olympia. After an unsuccessful attempt at negociation, the city was besieged in due form; and the attack commenced on Hexapylon by land, and on Achradina by water. But the Roman general was not successful at Syracuse, as he had been at Leontium: all his exertions were rendered nugatory by the talents of a single citizen; and the genius of Archimedes triumphed over the power of Rome.

\* Rollin's Ancient History, Vol. I. p. 302, 30, 04 De Non's Travels, p. 358, 359, &c. Diodorus Siculus, Lib. xiv. p. 279—295.

The Carthaginians seem to have been destined to owe the defeat of many of their most promising enterprises in Sicily to pestilential diseases. In a former part of the same war, whose unhappy termination has been described above, Hannibal, the predecessor of Imilcon, laid siege to the city of Agrigentum (Girgenti), in the south-western part of Sicily. For the purpose of raising a wall without, which should overlook and command the city walls, he collected all the materials within his reach, and, among the rest, destroyed, and converted to this use, the tombs standing round a city very ancient and populous, and then containing 200,000 inhabitants. From the uncovering and disturbing of so many dead bodies arose a terrible pestilence, which carried off immense numbers of the Carthaginians, and the general himself. Afflicted at this dreadful mortality, the besiegers attributed it, with the superstition of the age, to the vengeance of the gods, incensed against them for violating the repose of the dead. The healthiness of the situation, the season, and the thorough appointment and supplies of the Carthaginian army, leave no room to doubt as to the real cause of the sickness, which gradually disappeared: but two remarks, of some importance, are suggested by it. 1. The folly of modern nations, especially in warm climates, in suffering the interment of the dead within their cities. 2. The wisdom of some ancient nations, in having a *dead*, as well as a *living* town.

Necessitated

Necessitated to defer the further operations of the siege, the consul directed his arms against several other Sicilian cities, and struck his enemies with terror by his brilliant achievements. About the same time the Carthaginian general, Himilco, arrived at Agrigentum, to the aid of the Sicilians, with an army of 25,000 foot, 3000 horse, and twelve elephants. Hippocrates, one of the Syracusan leaders, with a part of the Syracusan troops, marched out to meet him; Epicles having been left, with the remainder, for the defence of the city. After some skirmishes with the Sicilian army, in which he was fortunate, Marcellus returned to Syracuse, whither he was soon followed by Himilco and Hippocrates, who, having formed a junction, fixed their camp at the river Anapus, about eight miles from the city. Nothing of importance occurred while the armies lay near each other. The combined chiefs soon drew off their forces; the Carthaginian general took up his winter residence at Agrigentum, and the Sicilian at Murgantia. Marcellus, who had been for some time busied in the interior, now again returning, appointed Crispinus to the command of the ancient camp at Olympia, and built and fortified a camp at Leon, on the eastern side of Syracuse, for himself.

The Romans commenced their operations early in the spring. Some of them having gained an entrance into Syracuse in the night, by stratagem, the gate at Hexapylon was broken open, and Marcellus entering, secured possession of Epipolæ before any effectual resistance could be attempted by Epicles. Tyche and Neapolis surrendered at discretion; the fortress of Euryalus speedily submitted; and the consul disposed this part of his army against Achradina, in three places, hoping to carry it by attack, or subdue it by famine.

While these preparations were going forward on the part of the Romans, Himilco and Hippocrates suddenly returned to Syracuse, and encamped on the great harbour. From hence they attacked Crispinus in the ancient camp of the Romans, while Epicles, in concert, sallied out from the city upon the posts of Marcellus. Neither of these assaults succeeded. Crispinus repulsed the allies, and pursued them tri-  
umphantly

umphantly to their station, at the same time that Marcellus compelled Epicides to take refuge within the walls of Achradina. After this, both of the Roman commanders strengthened their encampments.

Such was the progress and state of the war when a pestilence, common to both armies, appeared both in the camp of the allies and of Crispinus, and naturally diverted their attention from hostile operations: for the autumnal season, their unhealthy situation, and the heat, (much more intolerable without than in the city,) affected almost every person in either camp. At first they became sick, and died, simply from the effects of the season and the unhealthiness of their situation: afterwards, says the historian, the care and contact of the sick spread the disease; so that those who were attacked with it perished, neglected and forsaken, or their attendants fell victims to their humanity. Deaths and funerals passed before the view in rapid succession, and day and night resounded with lamentations. At length these scenes of calamity became so familiar, that they not only neither wept, nor lamented the dead, but even ceased to remove, or yet to inter them. Their lifeless bodies lay extended in heaps, in the very sight of those who expected a similar fate; while the dead infected the sick, and the sick those who were in health, as well with fear as with the corruption and pestiferous exhalations from the bodies: so that, impatient of life, and desirous rather to fall by the sword, some singly invaded the stations of the enemy. The plague raged with far greater violence in the Carthaginian than in the Roman camp; for the Romans, from long residence in the vicinity of Syracuse, had become accustomed to the air and water. As soon as the Sicilian auxiliaries of the Carthaginians perceived that the disease spread from the insalubrity of the place, they betook themselves to their nearest cities; but the Carthaginians, admitted into no city, together with their generals, Himilco and Hippocrates, totally perished. Marcellus, observing how fast the disorder increased, drew the remainder of his troops into the city, where, under shelter, and in the shade of the houses, they regained health and vigour, notwithstanding



withstanding many of the Roman army were carried off by the same pestilence \*.

Thus the Lysimilian marshes triumphed over a third army; and a number not less than 30,000 were added to those who had fallen of the soldiery of Nicias and of Imilcon. The practical consideration of these events belongs to the succeeding Section.

SECT. III.—The simplicity and uniformity of the preceding statements and narrative, preclude the necessity for laborious investigation and argument. The deductions to be made are unavoidably simple and uniform; but their force and tendency may be more distinctly perceived from a methodical arrangement.

1. Climate and season.—The climate of Sicily in general, and particularly of the city of Syracuse, is remarkably pleasant during a considerable part of the year. But the summers are excessively sultry; and this heat, operating on marshy situations and stagnant waters, never fails to engender malignant fevers. To these they are peculiarly liable in the summer and autumnal months. The pestilential diseases which proved so fatal to the armies of Nicias and Imilcon, and to those of the Carthaginians and the Romans, occurred either in the autumn or in the height of summer; and, in every instance, in the immediate vicinity of Syracuse.

2. Situation.—The camp of Nicias was near or at Plemmyrium; of Imilcon, at Olympia; of Crispinus, at Olympia also; and of Himilco and Hippocrates, at the head of the great harbour. The whole of this territory is a vast marsh, but partially covered with water even in spring, extremely offensive in summer, and noted, from time immemorial, for its insalubrity.

3. Condition of the armies.—The Athenian army was in constant action, and constantly unsuccessful. The army of Imilcon was in perpetual movement, but fortunate, and full of courage. The adverse armies, at the third siege, were in a state of inaction. These are certainly considerable dif-

\* Tit. Liv. typis Barbore, 1775. Tom. III. Lib. xxiv. & xxv. p. 303.  
—403.

ferences: why should they not modify the event? It may be doubted whether any moral cause would be sufficient to protect, for a long period, an unaccustomed resident in a marshy situation from the usual consequences. Fatigue and despondency are certainly favourable to the access of disease; and inaction, particularly in camps, is generally admitted to be scarcely less so. The uncommon heat of the year of Imilcon's irruption into Sicily, may be a satisfactory explanation why his successful troops were so readily affected by the unwholesomeness of his camp.

4. Progress of the disease.—The sick, in the army of Nicias, were not numerous at first, but the number gradually increased. This is attributed, by Plutarch, to contagion; but Thucydides, a cotemporary and more sagacious historian, expresses no such opinion, nor does it seem probable. The growing desperation of the Athenian affairs, and the tendency of the season to augment the extent and heighten the virulence of marsh exhalations, sufficiently account for the increasing predisposition to sickness, and for its more general prevalence in their army.

The same reasoning will apply, generally, to the case of Imilcon's army; but there were, in this instance, additional causes for the production of such immense mortality. The commencement of this pestilence was among his African auxiliaries. The circumstances of their impressment into the service, and of the contempt in which they were held by the Carthaginians, render it probable that less care was taken to accommodate them than the others. Some peculiar habits of body, modes of life, or varieties of native climate, might influence this event; or they might have been previously subjected to greater fatigue. Be this as it may, the sickness soon became universal, and so mortal that the dead lay unburied. In a situation like this, there is no reason to believe that much attention was at any time paid to the necessary duties of cleanliness. When the sick could find no attendants, and the dead none to inter them, it requires but little sagacity to infer that no means were used for the removal of excrementitious matters. And as a large proportion of this army were afflicted with dysentery, and the number of putrefying

trelying carcases must have been immense, and as the limits of the camp were not very extensive, and they were now sorely pressed by a vigorous enemy, incapable themselves of resistance, and suddenly precipitated from triumph to inevitable defeat and shame, we shall find no difficulty in assigning causes sufficient and satisfactory for the extraordinary fatality of this pestilence.

The third plague commenced at the same time in both the Carthaginian and Roman camps. At first, as in the former instance, the sick were attended to; but the number soon became so great as to render this impossible. In this case, as in the other, the dead were left unburied, and to putrefy; and in this case, as in that, there is no reason to doubt but that the causes and fatality of the diseases were in consequence exceedingly augmented. It may reasonably be doubted, however, whether the historian is accurate in ascribing any part of the spread of the pestilence to contact of the sick. The belief of this doctrine has been of long duration and extensive prevalence; but late observations do not seem to countenance its validity. To establish it, would inevitably annihilate a distinction much insisted on by the advocates for the importation of pestilential diseases into the United States; but this argument is not wanted for that purpose. It is more probable that the pestilence, in this instance, as in that preceding, was rendered more general by the increasing quantity and concentration of marsh effluvia; by the excrementitious matters in the camps, suffered to assume, unremoved, a putrescent action; and by noxious exhalations from putrefying bodies. There are no facts which authorise the opinion that a specific matter emanates from any part of the body, during pestilential or any other fevers properly so called, capable of generating a similar or a febrile disease. The most that we may venture to infer from all the facts known to us, is, that the perspirable matter, like any other animal substance, when separated from the living body, or deprived of life, is liable, under certain circumstances of temperature and moisture, to undergo a putrefactive fermentation, or to go through such changes as shall adapt it for the production of febrile diseases; and this,

as well when suffered to lie on the skin, as when elsewhere, or otherwise disposed of. With these limitations, and in this view of the question, there need be no objection to the testimony of Livy; nor have we any ground, from a knowledge of the symptoms of the plague he describes, to oppose or fortify his narration.

5. Mortality of the pestilence.—Of the mortality of the sickness which affected the army under Nicias, we only know that it was considerable. Our knowledge is not much more definite respecting the number of the Carthaginians who perished of the army of Imilcon. There is reason, however, to believe that it far exceeded that of the Athenians, and probably it was not less than 100,000 men. For it must be recollected, that many had been regularly interred in the first of the plague, and that all the sick and wounded were left behind when their general ingloriously fled from Sicily. Nor is it probable that a number greatly exceeding 50,000 fell in the battle which preceded his flight. In the last case, our computation may be nearer the truth. The army with which Himilco invaded Syracuse amounted to 28,000, exclusive of his Sicilian auxiliaries, and of the troops from that city which joined him under the command of Hippocrates. The defeat of this last, by Marcellus, the preceding year; the secession of the Sicilians; and his own losses in the various actions in which he had been previously engaged, had no doubt reduced his army below its original strength. But, when it is remembered that all this army perished, and that a less, but still a great number of the Roman army also were destroyed by the plague, there is reason to estimate the whole loss as exceeding 30,000. The entire number of men sacrificed to this unhealthy situation, in three successive periods, could not be less than 150,000: a most melancholy illustration of the influence of climate, season, and soil, on the health of mankind; and an example of the activity of local causes in producing disease, compared with which, all the yellow fevers of the United States, whether originating here, or imported from abroad, scarcely deserve to be remembered.

6. Symptoms and nature of the pestilence.—On this point our information is less complete than on any other.

The sickness in the Athenian army is simply called a fever, said to be contagious by Plutarch, but probably not so. Livy describes no symptoms of the plague recorded by him. Diodorus Siculus is somewhat more particular; but only so far as to mark the variety of form in which the soldiers of Imilcon were attacked by the disease. In some, in the shape of enteritis, or inflammation of the bowels; in others, of dysentery, sometimes invading as a violent fever, with acute pains in every part of the body, and sometimes with madness or delirium. Yet, even these scanty particulars are of importance, as they serve to identify the disease, and to proclaim its strict affinity to those which are the common offspring of such situations as that in which the army were encamped, and those which have spread so much apprehension, and excited so much discussion in our own country.

7. Cessation of the pestilence.—Another circumstance which characterises the disease under consideration, is the manner in which it was extinguished. So long as Nicias remained in his camp, he saw his men constantly dying around him by sickness. His removal, notwithstanding the subsequent calamities which befel him, appears to have thoroughly delivered his army from fevers. The survivors were employed as slaves in Syracuse; which could scarcely have happened had they been sick, or had the Syracusans dreaded the introduction of a contagious disease by their means.—Imilcon preserved the residue of his army only by flight: there was no other hope for their safety; and, after his return to Carthage, he exclaimed, in the bitterness of his grief, that the plague, not the enemy, had conquered him! But neither then nor before did the dry and airy city of Syracuse suffer from any sickness; nor did the fear of contagion prevent the Syracusans from repeatedly attacking the Athenian and Carthaginian camps. Their minds were probably unwarped by the bias of system; and they discerned, in the position of their enemies, the true cause of their misfortunes.—The facts related by Livy are singularly precise and important. In the first place, neither the citizens of Syracuse, nor the army of Epicles, which had possession of Ortygia and Achradina,

nor that part of the Roman army which, under the command of Marcellus, was stationed in the elevated quarters of Tyche, Neapolis, and Epipolæ, were in the least affected by the plague. But this calamity was limited to the Romans under Crispinus, in the ancient camp at Olympia; and to the allies, who were encamped at the head of the great port. Crispinus and his troops, who had lived at Olympia a considerable part of the two preceding years, or at least had remained in the vicinity of Syracuse, and had become in a degree habituated to the air and water, suffered less severely than their enemies; and, when the sickness became general among them, recruited, and ceased to be taken sick, in consequence of a removal to the high grounds of the city, occupied by their countrymen. The natives of the island, seeing the danger to which they were exposed, took refuge in their own cities, and escaped the disease; but the Carthaginians, without any place of refuge, and entirely unaccustomed to their situation, totally perished.

8. On the whole, then, it appears that the mortality, in every instance, occurring in the armies near Syracuse, originated entirely from local causes: that there is no reason to suspect that this mortality was heightened by contagion, in the usual acceptation of that term: that the symptoms, so far as we have any account of them, were similar to those which occur, under similar circumstances, in the East and West Indies, in our own country, and in every part of the world: and that, as no difference is recorded as existing between these great epidemics or endemics before and after the causes were increased by animal putrefaction, the advocates for the foreign derivation of pestilential diseases must relinquish the distinction between fevers from animal and vegetable putrefaction; or, if they maintain the contagious quality of the former, must admit, on equal evidence, the same quality as characterising the latter. To this it may be added, that, as it was the universal practice of the ancients to designate any and every wide-wasting disease by the name of plague, without any special reference to its peculiar symptoms, so there is an equal propriety in our conferring the same appellation on  
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our dysenteries and fevers; nor do these less deserve the title than the fevers of Smyrna, Cairo, and Constantinople, which resemble them in their origin, their varieties of prevalence and degree, their appearance and disappearance, and which would, in all probability, yield to the same regimen and remedies.

VIII. *Observations on the Art of Scouring different Kinds of Stuff.* By C. CHAPTAL\*.

**T**HIS art supposes, 1st, a knowledge of the different substances capable of staining any kind of cloth; 2d, of the substances to which recourse must be had in order to make those deposited on the stuff to disappear; 3d, a knowledge of the effects produced on colours by those re-agents which it may be necessary to employ to destroy stains; 4th, a knowledge of the manner in which the cloth is affected by those re-agents; 5th, of the art of restoring a colour changed or faded.—Of those bodies which occasion spots on different kinds of cloth, some are easily distinguished by their appearance, such as greasy substances; but others have more complex effects, such as acids, alkalies, perspired matter, fruits, urine, &c. Acids redden black, fawn, violet and puce-colour, and every shade communicated with orchilla-weed, iron, astringents; and every blue except indigo and prussian blue. They render the yellows paler, except that of arnatto, which they change into orange.

Alkalies change to violet the reds produced by Brazil wood, logwood, and cochineal. They render the greens on woollen cloth yellowish, make yellow brownish, and change the yellow produced by arnatto to aurora. Perspired matter produces the same effects as alkalies.

When the spots are produced by simple bodies on stuffs, it is easy to remove them by the means already known. Greasy substances are removed by alkalies, soaps, the yolk of eggs, fat earths; oxyds of iron, by the nitric and oxalic acids; acids by alkalies, and reciprocally. Stains of fruit on white

\* From the *Bulletin des Sciences*, Vol. II. No. 4.

stuffs may be removed by the sulphurous acid, and still better by the oxygenated muriatic acid. But when the spots are of a complex kind, it will be necessary to employ several means in succession. Thus: to destroy the stain of coom from carriage-wheels, after the grease has been dissolved the oxyd of iron may be removed by the oxalic-acid.

As colours are often changed by re-agents, it will be necessary, in order to restore them, that the scowerer should possess a thorough knowledge of the art of dyeing, and how to modify the means according to circumstances. This becomes the more difficult when it is necessary to re-produce a colour similar to that of the rest of the stuff, to apply that colour only in one place, and often to restore the mordant by which it was fixed, and which has been destroyed, or even the first tint which gave the colour its intensity. It may be readily conceived that the means to be employed must depend on the nature of the colour and the ingredients by which it was produced; for it is known that the same colour may be obtained from very different bodies. Thus, after an alkali has been employed to destroy an acid spot on browns, violets, blues, poppies, &c. the yellow spot which remains may be made to disappear by a solution of tin; a solution of sulphat of iron restores the colour to brown stuffs which have been galled; acids restore to their former splendour yellows which have been rendered dusky or brown by alkalies; blacks produced by logwood become red by acids; alkalies change these red spots to yellow, and a little of the astringent principle makes them again become black. A solution of one part of indigo in four parts of sulphuric acid, diluted with a sufficient quantity of water, may be employed with success to revive the blue colour of cotton or wool which has been changed. Scarlet may be revived by means of cochineal and a solution of the muriat of tin, &c.

The choice of re-agents is not a matter of indifference. Vegetable acids are preferable; the sulphurous acid however may be employed for stains occasioned by fruit: it does not change the blue of silk nor colours produced by astringents: it does not degrade the yellow of cotton. Ammonia succeeds better than fixed alkalies in removing spots produced by acids.

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It is employed in vapour; its action is speedy, and seldom alters the colour.

The means of removing greasy spots are well known. This effect is produced by alkalies, fullers' earth, volatile oils dissolved in alcohol, a heat proper for volatilizing grease, &c. Spots occasioned by ink, rust, or iron-mould of any kind, and all those produced by the yellow oxyd of iron, are removed by the oxalic acid: the colour may be restored by alkalies, or a solution of the muriat of tin. These spots may be removed also by the oxygenated muriatic acid, when they are on white stuffs or paper.

The action of alkalies, and that of perspired matter, are the same: their spots may be effaced by acids, or even by a weak solution of the muriat of tin. When these spots arise from several unknown causes, in order to destroy them recourse must be had to *polychrest* compositions. The following may be considered as one of the most efficacious. Dissolve white soap in alcohol, and mix this solution with the yolks of from four to six eggs: add gradually essence of turpentine; and incorporate with the whole some fullers' earth, in such a manner as to form balls of a suitable consistence. Moisten the spot; and, having rubbed it with these balls, the spot will be removed by washing the stuff. All spots, except iron-mould and ink, may be removed in this manner.

Washing destroys the lustre, and leaves a tarnished place disagreeable to the eye; but the lustre may be restored by drawing over the washed place, and in the direction of the pile, a brush moistened in water impregnated with a little gum. You may then apply a sheet of paper, or a piece of cloth, and a considerable weight, under which the cloth must be left to dry.

*IX. Description of some Improvements invented by Mr. JAMES BURNS, of Glasgow, applicable to Fire-Grates, Stoves, &c. by which Rooms and Halls of every Description may be heated more speedily and effectually, and with a smaller Waste of Fuel, than by the Methods now in Use\*.*

THE principal merit of Mr. Burns's improvements, and it is not a trivial one, consists in his contriving to maintain the combustion of the fuel in *open* stoves or grates, without employing for that purpose the air of the room in which they are placed. The heat liberated and thrown out into the apartment is thus prevented from being unnecessarily wasted and hurried up the chimney, as is unavoidably the case in the usual method, where the combustion is maintained by air from the apartment, the waste of which is supplied by exterior cold air that comes pouring in at the bottom of the doors and the sides of the windows, thereby undoing a great part of the effect produced by the fire.

To prevent this waste, the air that maintains the fire in Mr. Burns's grates, or in others to which his improvements are applied, should be brought through a tube from the outside of the house †, or be made to pass from the outside of the house, between two of the joists, where the floors and ceilings are found enough to allow this, so as to be brought to the bottom bars of the grate, without having any communication with the interior air of the room, while at the same time the grates, and parts connected with them, should be so constructed, that, when the fire is not wished to be supplied with cold air from the outside of the house, the passage may be shut, more or less perfectly, by means of a valve, a small door, a cock, or any similar contrivance. When convenience does not admit of the air-tube being carried to the

\* Mr. Burns has lately obtained a patent for these improvements.

† The same principle had been already successfully applied to *close* furnaces: Mr. Howard's, of which we gave an account in our last Number, furnishes an example. But its application to common open fires is a new idea, and will probably produce an entire revolution in the construction of grates for common use.

outside of the house, if carried to a cellar, larder, stair-case, or any lumber-room, the same end will be gained; with this further advantage, that such cellar, larder, &c. will thereby be well ventilated, and prevented from acquiring or retaining any unhealthy or disagreeable smells.

The form of the grates may be varied indefinitely; but the one we are about to describe answers the purpose so well, and is at the same time capable of being made, by the aid of a good architect, so highly ornamental to any apartment, that our readers will fully comprehend the facility with which it may be accommodated to circumstances.

Fig. 1, (see Plate V & VI.) represents a grate of the improved construction, and Fig. 2 is a section of it. The air that maintains the combustion is supplied through the pipe or tube A, (Fig. 2 and Fig. 4.) or from between the under-ceiling and floor as before-mentioned, from which it passes up by the back or side of a drawer B, Fig. 2, according as it is most convenient to bring it up by the one or the other; the back however is preferred, where convenience admits of it. The front of this drawer, in its place, is seen in Fig. 1, at C. The intention of this drawer is to receive the ashes that fall from the bottom bars of the grate, sections of which are represented at DD, Fig. 2. The ashes, as they fall from the bars, strike against the sides of the internal cavity E, and then are carried by their own gravity through the narrow part F, and fall into the drawer. This part of the construction may be easily understood by conceiving it to be an hollow vase, as it actually is, with a drawer in its pedestal or basement, and having a grate over it, on which the fire rests. The valve, door, cock, or other contrivance by which the external air is admitted or excluded, may be either in the neck F of the vase, or in the air-tube, or in the drawer B or C, or, which is preferable, in the cavity behind the drawer, the pedestal or basement of the vase being made large enough to admit of such cavity. Various constructions may be resorted to; but that represented in Fig. 3, (which is a ground-plan of a proper chimney for such a grate, and in which a bird's-eye view of one half of the grate may be seen in its place, while the other half repre-

sents an horizontal section of the basement or pedestal, which is supposed to be open, shewing the drawer in its place, with the cavity behind it,) will be found to answer every purpose. The cavity G is separated from the place in which the drawer is put, by a partition (best made of plate or cast-iron) passing from H to H, in which there is an opening I, with a cover K. To the cover K is attached a small bar L, worked by another shorter bar M, to which it is attached by any simple joint. The bar M is fastened into an upright pivot, the top of which comes up through the pedestal at one of its corners or any other convenient spot, and is furnished with a button to be laid hold of by the finger and thumb, or with a top fitted to a key, by the turning of which the cover K is made to shut or open the aperture I, and consequently to prevent or facilitate the passage of the air from the hole at G, which represents the internal mouth of the air-tube, the other end of which is on the outside of the house, or in any cellar or other apartment, as before described. When the air has passed through the aperture I, it finds no difficulty in passing on to the bottom of the grate, that back or side of the drawer next which the aperture is being made low to allow it to flow in freely.

The grates are recommended to be made of a circular or elliptical form, as being most convenient, where the fence or screen, to be immediately described, is wished to be applied along with Mr. Burns's other improvements. The fence is intended to prevent those dreadful accidents, which too frequently occur, of women's and children's clothes being set on fire by sparks from the grate. When it is wished to be adopted, the inside of the chimney, where the grate is to stand, had best be a semi-cylinder, or nearly so, (as represented in Fig. 3,) with a lining or cover *aaa*, best made of metal, at such a distance from the semi-cylindrical wall, NNNN, as to give sufficient room for allowing the safeguard or fence to be slid round into it when the fire is wished to be left open, when fresh fuel is to be added, or when the drawer with the ashes is to be removed.

The fence is a frame-work of metal, which, when filled up with glass or with wire-work, forms a portion of a cylinder

der answerable to the curvature of the space between the back of the chimney and the lining above-mentioned, made in one or more pieces, and moving in a circular groove, *bbb*, in or upon the hearth, which serves to conduct it into its place behind the grate, when the fire-place is wished to be left open, as before mentioned. The top of the front of the opening of the chimney *O*, Fig. 4, projects in a circular form, or is furnished with an added projection, made of metal, marble, or any other fit material; but in either case the projection is furnished with a circular groove on its under surface, of the same radius with the one in, or planted on the hearth, for the purpose of receiving the upper part of the frame-work of the fence or safeguard, which frame-work may be filled up with glass, either plain or bent, and either colourless or coloured, or stained or painted with figures or designs of any kind. By this means complete safety is obtained against any such accidents as have been alluded to, while at the same time the comfort arising from the view of a cheerful fire is not prevented by the interposition of any opaque body; but for nurseries or the like, where convenience and safety is more the object than elegance or luxury, the frame-work may be filled up with wire-work. The fence may be seen in its place at *P*.

Where either the glass or the wire-work fence, or both of them, are meant to be applied to square or rectangular chimneys, without the trouble of giving them the semi-cylindrical form, the lining to receive the fence or fences may be introduced at the sides or jambs of such chimneys; or the fence may be made to rise by means of pulleys into the wall above the opening, or slide sideways into the walls at the sides of the opening.

These improvements may be introduced together or singly, and may be applied to many of the grates now in common use.

Besides the advantages already pointed out as connected with them, they possess also the following.—Any room or apartment may be heated by their means with a much smaller quantity of fuel than by any other method yet in use: at the same time the advantage of seeing the fire is not

lost, as in close stoves; for these grates have side as well as bottom bars, which allow the radiant heat and light to be thrown out into the room without any impediment: and in fact large rooms, halls, and the like, which by the usual methods can hardly be warmed, or made at all comfortable in cold weather, may, by means of these improvements, be heated as effectually as the smallest apartment; for, when their full effect is wanted to be produced, it is only necessary to keep the fence in its recess, that even that portion of heat, which would be kept back by the interposed glass or wire-work, may be thrown out into the room, and perform its office. They are also an effectual cure for smoky chimneys, which not only cause great waste and destruction of good furniture, but many diseases to the inhabitants of houses plagued with that evil. They cause a clean fire-side to be easily commanded at all times, as hardly any of the dust or ashes fall through the side bars, almost the whole passing through the bottom bars down into the drawer; and any fire lighted in such improved grates burns up and becomes lively in a few minutes, without the aid of bellows, and that watchful care which common grates or stoves require.

X. *On the Chemical Action of different Metals on each other at the common Temperature of the Atmosphere.* By C. FABBRONI\*.

THAT particular sensation, first made known by Sultzzer, which is felt on the tongue on bringing two metals into mutual contact, and which would have excited none if they had been applied separately to that organ, has been ranked among the galvanic phenomena. C. Fabbroni, however, instead of ascribing these effects to an agent almost unknown, such as the electric fire, is of opinion that it depends on a chemical operation, perhaps as the sensation of taste itself. He has endeavoured to prove the truth of this opinion by a number of observations and experiments.

\* From *Bulletin des Sciences*, Vol. II. No. 5.

He had remarked that several metals, such as mercury, tin, lead, retained their metallic brilliancy as long as they were pure, while compounds of them became soon tarnished and oxydated; that the mere contact of two different metals seemed to hasten their oxydation; and that, in this manner, the mixture employed for foldering the plates of copper which cover the observatory of Florence, had been speedily changed into a white oxyde at the outer edge of its contact with the copper, &c. He thinks that, in this case, the metals have a reciprocal action on each other; and that this action, more efficacious and more sensible when the attraction of aggregation of the metals is destroyed by fusion, exists no less between solid metals when they touch each other.

If the effects, as rapid as those of lightning, occasioned on the tongue by two metals brought into contact, have been by some ascribed to a peculiar fluid not galvanic, or to the electric fluid, it is because they did not recollect that chemical action is exercised between two bodies with the utmost rapidity. The signs of electricity which have been sometimes observed on separating two metals in contact, are rather the consequences than the cause of that action; for it is known that the greater part of chemical operations change the electric equilibrium of bodies, and must consequently give birth to electrical phenomena. Without totally excluding electricity, therefore, from all galvanic facts, C. Fabbroni thinks that this fluid has some share in the sensation experienced by the tongue from two metals in contact. This action of metals in contact is proved by the following experiment:—

C. Fabbroni put into two glasses filled with water, pieces of different metals, one in each glass. In other glasses he put two pieces of different metals, but kept them from touching by interposing a plate of glass. In a third series of glasses he also put two pieces of different metals, but in contact. In the metals of the two first series he observed no change, while the most oxydable metals of the third series were covered with an oxyde, which considerably increased in the course of a few days, and the metallic pieces even contracted a strong adherence. The quantity of the caloric which disengages itself in these combustions is too small to be measured, yet the light

light which emanates from it may be seen, if the eye itself forms a part of the experiment, by holding a piece of silver in the mouth, and applying a bit of tin to the ball of the eye. After these two metals are made to communicate, you see a feeble but distinct light, which disappears at the end of a few seconds, because the eye becomes accustomed to this feeble sensation; but it may be renewed by drawing the metal over the opaque cornea, and then over the transparent. The author ascribes to a convulsive sensation that kind of flash which some persons think they perceive \* at the moment of the contact of these metals, applied one on the tongue, and the other under it.

To make the oxydation of the two metals take place under water, the presence of air is also necessary. C. Fabbroni thinks that air is useful in this circumstance, to add to the water a sufficient quantity of oxygen to be taken up by the metal, as silver is added to gold in order to perform the operation of quartation. If a piece of silver and a piece of tin be put in contact in water, inclosed in a flask of flint-glass hermetically sealed, the tin is oxydated; but the oxyde of the lead of the flint-glass is decomposed, and the glass becomes black. Philosophers, who ascribe these phenomena to electricity, bring, as a proof of their opinion, that they take place when the metals are connected even by means of a pretty long chain. C. Fabbroni has determined the length of this chain at about 18 or 20 feet. Beyond that distance these phenomena are no longer sensible; while, on the other hand, the electric fluid is communicated to indefinite distances.

If the phenomena of the experiment of Sultzzer belonged really to electricity, they ought to take place with all metals, whatever may be the relation of these metals to each other. C. Fabbroni mentions a great number of these combinations which produced no effects; and other combinations, of the same metals, which produced sensations very distinct. Thus, if silver be placed on the eye, and gold on the tongue, making them communicate by means of copper, the sensation is almost nothing; but it becomes very evident if the iron touches

\* The perception is as real as that induced by drawing the metal over the cornea of the eye. EDIT.



the eye, and the silver the tongue, the communication being established as before with copper\*.

In regard to the hydrogen of the decomposed water, the author thinks that it may also be absorbed by the metal: he even considers as an hydro-oxyde of tin, the octaedral crystals which he remarked on the surfaces of the pieces of tin employed in these experiments.—It is clearly seen, says C. Fabroni, by the results which I obtained from the simple contact of two metals in water, that is to say, by the oxyde and saline crystals, that a chemical operation takes place, and that to it we ought to ascribe the sensations experienced on the tongue and by the eye. It appears to me, then, probable, that it is to these new compounds, or their elements, that we are indebted for that mysterious stimulus which produces the convulsive movements of the animal fibre in a great part at least of the galvanic phenomena.

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XI. *On the Art of Hardening Copper.* By P. I. HJELM †.

COPPER, in its pure and perfect state, is exceedingly soft and malleable: its toughness is then so great, that it is exceeded only by gold and iron. When copper is hammered a long time cold, and still more when rolled, it is found, as is well known, somewhat harder than before, but it does not thereby acquire that strength which deserves the name of hardening, or which enables it to make sufficient resistance to strong impressions. By being brought to a bright-red heat in the fire, and suddenly quenched in water, copper obtains no perceptible addition to its hardness; but, on the contrary, becomes more pliable, and consequently softer than before. If the copper is kept a long time in fusion, or often fused in a strong heat, without any covering of flux or charcoal powder, it becomes brittle and unmalleable, and consequently

\* This is no conclusive argument against the effect being electrical: it is taking for granted that the laws and properties of the electric fluid have all been ascertained. EDIT.

† From *Transactions of the Royal Academy of Sciences at Stockholm* for 1797.

harder : but these properties are soon lost when the copper is melted in contact with carbonaceous matter. If melted copper be poured into water, as is done in the operation of granulating or corning, it does not appear that it acquires any perceptible degree of hardness, or such as can be compared to that which is communicated to steel by the same means.

Considering all these circumstances, which have been confirmed by experience, it could not but excite attention to find, by the most undoubted testimony, that the ancients actually possessed the art of hardening copper, which they employed for instruments of all kinds ; such as daggers, swords, bows, shields, javelins, &c. Though ancient authors often mention these weapons, none of them have given us any account of the method of hardening the copper. This deficiency some have attempted to supply by conjectures, which have given rise to a variety of experiments, but not one of them was ever attended with success. As it was imagined that in hardening copper it would be necessary to follow the same process as that employed for steelifying iron, most of their researches have been directed to a similar method ; and for that purpose they have not only prescribed, for the hardening of copper, such processes as are employed for hardening iron when it is to be converted into steel, but even the most absurd methods founded upon these processes. These researches, however, instead of answering the intended purpose, only tend to shew the ignorance of those who think they find in the greatest absurdity the most important secrets, and to involve us in still greater darkness.

The art of hardening copper has therefore of late been considered as one of those known to the ancients, which were afterwards lost ; and it would no doubt have still remained in that state, had not several of these monuments of our ancestors been brought to light by some fortunate accidents, and the respect for their great antiquity been overcome by a desire for becoming acquainted with their composition. When this method, the only certain and decisive one, was once chosen, it was no great difficulty to discover that the whole art did not depend on any process like that employed in regard to iron, but on the addition of a certain quantity of

some other metal melted with the copper, by which it was rendered harder than before.

The abbé Mongez first wrote a treatise on the metallic composition of the ancient bells, which he transmitted to the Academy of Inscriptions. He found that this composition was in general nearly the same as that still used for bell-metal, that is, copper and tin. M. Mongez afterwards transmitted to M. Dizé the point of an ancient copper dagger, the appearance of which on the fracture sufficiently shewed that it had been fused and cast. When dissolved in pure nitrous acid, there remained a white powder, found to contain tin, and that which had been dissolved was merely copper. It evidently appeared, therefore, that the supposed art of the ancients for hardening copper, was nothing else than fusing it with a certain quantity of tin. This was still farther confirmed when M. Dizé found the same component parts in some Greek, Roman, and Gallic coins, which he obtained on the same occasion for the purpose of examination. The tin in them formed about twenty-four parts in a hundred of the mixture.

In support of this opinion, respecting the art of hardening copper among the ancients, I have the honour of laying before the Academy some experiments lately made on this subject.—Last summer, Professor A. I. Retzius, of Lund, transmitted to me a part of the blade of a two-edged dagger, which, together with some stone-cutters' chissels, were found in a hill of earth on the low lands of Scandinavia, where whole swords of ancient workmanship have often been found. This fragment, on the outside, had rather a yellowish appearance like brass, than the red colour of copper; the edge was exceedingly thick, and roundish; the fracture seemed also granulated, which evidently shewed that the work had been cast. When tried by the file, it was not quite so hard as the common bell-metal, but somewhat harder than common gun-metal. The newly-filed surface had a reddish-yellow appearance, but soon became yellower. When melted by the blow-pipe, this compound metal exhibited no traces of zinc; but it might be readily seen, by its appearance, that the greater part of the mixture was copper. The filings were

not in the least attracted by the magnet, which clearly proved that there was no iron in the composition. It exhibited as little traces of any other metals, as far as could be judged from the ascending vapour. In order to ascertain with what metal the copper had been mixed, twenty-five assayer's pounds of the purest filings of the above fragment were collected; pure nitrous acid was heated in a glass retort, and diluted with distilled water; and a few of the filings were thrown into it: when the first quantity was dissolved, a few more were added, and this was continued till the whole quantity was put in. The solution was made to boil for a quarter of an hour, and then diluted with a little more distilled water, in order that a white powder, which remained undissolved, and which I suspected to be calx of tin, might more readily fall to the bottom. After this was done, the bright blue-coloured solution, which was found to contain nothing but copper, was carefully poured out, and distilled water was poured over the white powder, which was washed several times in the same manner, after it had each time remained at rest long enough to allow it to settle, when it was thrown upon filtering paper to be farther washed. This white powder, when dry, weighed  $5\frac{1}{2}$  assayer's pounds, which gave  $21\frac{1}{2}$  pounds of tin calx in the whole hundred. Now, tin calx loses, by being revived,  $\frac{1}{4}$ , or 25 *per cent.* of its weight; and therefore this  $21\frac{1}{2}$  pounds of tin calx must have given  $16\frac{1}{8}$  pounds of metallic tin in the hundred, which, in an experiment made for the purpose, was found to be nearly the case, and the regulus obtained was found to be pure. The metallic compound of the before-mentioned dagger blade was made therefore of  $83\frac{7}{8}$  copper, and  $16\frac{1}{8}$  tin; or, to reckon without fractions, of 84 parts of copper, and 16 parts of tin.

By way of experiment, a compound of this kind, consisting of pure copper and pure tin, was fused, and a pen-knife blade was made of it, which was polished and ground in the usual manner. This blade had all the properties and the external appearance of the fragment of the dagger. Where the edge was thin, it was easily turned: it was therefore thought that perhaps, in the dagger before mentioned, the proportion of tin was greater. To ascertain this point, another composition

was made, of 20 parts of tin and 80 of copper, and a knife blade was made of it, as before. This was much whiter and harder, but also brittler in the same proportion, and therefore broke by carelessness in the polishing. The edge, however, was so sharp that it could be used for making pens; but it did not stand long, as notches were formed in it by each cut.

When the tin makes twenty-five hundred parts in the mixture, it becomes rather white than red, but exceedingly brittle. If the addition of tin be increased to thirty in the hundred, or more, both these properties are increased in the same proportion, and the composition becomes fit for specula. In gun-metal tin makes nine parts, or more, in the hundred: in bronze, 84 parts of copper are mixed with about 16 parts of tin, but a considerable portion of zinc or brass is sometimes substituted for the latter. Bell-metal contains in general 76 parts of copper, 19 of tin, and 5 of brass, or thereabouts.

What has been here said may perhaps be sufficient to confirm the opinion respecting the art employed by the ancients to harden copper, and may furnish some hints for the employment of such compounds in common life. Besides, we are hereby enabled to appreciate the different opinions entertained on this subject. M. Monnet imagined that the copper in ancient times was mixed with arsenic, which rendered it hard. No real objection can be made to this being possible; but as long as no ancient implements made of this mixture are found, the above assertion may at any rate be doubted, without mentioning other circumstances which seem directly to oppose it.

M. Dizé \* mentions the addition of iron to copper, as the means of rendering the latter harder; and endeavours to prove that Geoffroy the younger, who first drew this conclusion, (from an experiment he made, where copper, mixed with sixteen parts of iron in the hundred, was found to be equally hard, to have the same grain on the fracture, and to be as fit for making cutting instruments as the hardened copper of the ancients,) was too precipitate in forming his

\* *Journal de Physique* 1790, April.

opinion. The question, however, assumes a different appearance, when we take into consideration the experiment as related by Count Caylus, in his work on the Egyptian, Etruscan, Greek and Roman antiquities\*: for it there appears that M. Geoffroy undertook the above-mentioned experiment by the desire of Count Caylus, who describes various kinds of arrows and javelins of ancient workmanship, which, though they had the appearance of copper, were mixed with iron; because filings of them were attracted by the magnet; because the fracture had a different appearance from that of other instruments made of the hardened copper of the ancients mixed with tin, and was at the same time less fusible. M. Geoffroy thinks it rather remarkable that mankind should so early have fallen on the method of uniting copper with iron in an uniform mixture, which even at present is considered as a difficult process. It is well known that the most common copper ore consists of copper and iron mineralised with sulphur, and which is called pyrites of copper; not that the copper makes the greater part of it; but because the copper is of the greatest value, though the iron seems most generally to constitute the principal component parts. When this ore is smelted, the first copper obtained, or the so called black or raw copper, is necessarily rendered impure by a greater or less quantity of iron, according as more or less care has been employed to separate it during the operation. It is therefore in our power, it is said, not only to obtain copper combined in this manner with as much iron as may be necessary, but also to cast all kinds of instruments of it, and afterwards to hammer them cold, or to expose them to the same process of hardening as if they were of pure steel. Some assert that this method has been attended with complete success. Count Caylus tried also to harden pure copper by melting; but, instead of becoming hard, it was found softer and more malleable, which agrees with what has been said in the beginning of this paper.

Without in the least lessening the credibility of this assertion, which seems to have great probability in its favour, we

\* Recueil d'Antiquités Egyptiennes, Etrusques, Grecques et Romains. Vol. I. p. 238—251.

may at least exprefs a wish that fome person would make, of copper mixed in this manner with iron, different instruments, and then endeavour to shew real antique works made of the same mixture, in order that they may be compared. Until this be accomplished, it will be best to adhere to that process which seems to have been chiefly followed, and of which indubitable proofs are still in existence.

The history of the antiquity and use of metals in the period to which this question properly alludes, is involved in so great darkness, that nothing decisive on the present subject can be derived from it. This much, however, is certain, that the works of ancient authors still extant speak of gold, silver, copper, iron, tin, and lead, as known at the same time, and employed for various purposes. In regard to copper in particular, it is found more abundantly in a native state than any other metal, and requires nothing farther than smelting to be immediately used. In process of time mankind would become acquainted with the art of extracting the metal from other ores of copper; not, however, without greater labour, and therefore at first none but rich ore could be used; and we may with justice conclude, that copper was one of the first metals worked.

Tin is not found in a native state, but its ore is abundant in certain places, and is easily revived, or brought to the metallic state, especially when people are acquainted with the process of smelting other metals. The antiquity of tin, therefore, is as well established as that of the other metals among which it is named. In the time of the Tyrians a considerable trade was carried on in this metal, which they brought from the Cassiterides islands, beyond the pillars of Hercules, under which name England is no doubt meant.

Whether the art of mixing these two metals together by fusion was first found out by accident, or by experiments made for the purpose, it is not to be doubted that cast works of such a composition are mentioned at the same period with the simple metals. Besides, the works in bronze of the ancients are a sufficient proof of their skill in combining metals, of their art of modelling after Nature, and of their  
readiness

readiness in casting. All their instruments and edged tools of this kind hitherto found have been cast, and not hammered; and of this kind, without doubt, were the instruments mentioned by Professor Pallas as having been found near the Tschudi mines. All of them, except a few, consisted of a composition of copper and tin.

The art of preparing and separating iron may have been discovered as early as that of preparing other metals. As this art, however, requires a greater degree of dexterity, it was doubtless less common at first, and must have been diffused more slowly. This must have been the case much more with the art of preparing steel, which naturally would be much later than that of preparing iron, and melting tin and copper. The method of preparing steel may have been generally known in one country before it was communicated to another, where copper, hardened in the above manner, may have been used in its stead. The Japanese still use mirrors of white copper, which consists of a mixture of that metal and tin. The metal of the Chinese gongs consists of copper, mixed with 18 parts in a hundred of tin, and probably a little nickel; and these instruments are at the same time subjected to strong hammering when cold. The Chinese form their ill-shaped razors of iron (not hammered) filed to an edge, and which cut so badly, that, after every stroke on the beard, they must be drawn over a file. Many tribes who have been discovered in modern times, and who are unacquainted with the use of metals, employ hard kinds of stones for making knives, hammers, arrow-heads, axes, &c. Industry always supplies itself with its necessities, and employs for that purpose such materials as can be obtained, without taking into account the degree of labour which the preparation of them may require.



XII. *Experiments on the Nature and Properties of the Pietra fungaja, Lapis fungifer* \*. By P. A. GADD †.

STONES in general are in Italy called *Pietra*; a word which sometimes is used to denote the harder sort of stones, as well as stones of a certain genus; for example, *Pietra bigia, obsidiana, nephritica*, &c.; and this is the case with the so called *Pietra fungaja*. I. I. Ferber may be considered as the first person who gave a description of it in his letters from Italy. He has remarked also that a kind of it is in common use in the houses of Naples and Rome; and that he saw another kind in the possession of M. Fabbroni, at Florence: the first kind, which was found in the chalk-hills near Naples, consisted of white calcareous stalactites, and a number of small roots of vegetables; the latter was a hardened turf, dug up in the neighbourhood of some volcanic mountains.

A few years ago, M. Charles Spärre, chancellor of the academy, having been so kind as to transmit to me a piece of *pietra fungaja*, which he had brought with him from Italy. I analysed it, and found the result as follows:—It burns in an open fire, and emits the smell of putrid vegetables. When burnt in a strong fire, the greater part of it becomes dark-grey ashes. A hundred parts of this hardened turf, lost about fifteen parts in weight. When fused in a stronger heat, it is converted into a black opaque slag, which, however, is difficult to be fused. When a small part only of this earth is fused with borax, the glass acquires a dark-green colour. If a little water be poured over the calcined earth, the water exhibits traces of dissolved pot-ash, but the earth does not appear to have thereby sensibly decreased in weight. One hundred parts of the earth, previously calcined in a crucible, being analysed, were found to contain about 45 or 46 of siliceous earth, 23 argil, 7 calcareous earth, 20 calx of iron, together with traces of magnesian earth and pot-ash.

When the *pietra fungaja* is kept in a cellar, and moistened

\* Mushroom-stone, or mushroom-bearing stone.

† From *Transac. Royal Acad. Stockholm* for 1797.

with water, it produces a great many eatable mushrooms, which in Italy are served up at the tables of the great as delicacies. It needs excite no wonder that mushrooms should grow on the *pietra fongaja*, since a multitude of fruitful mushroom seeds are intermixed in this soft stalactites, as well as with the hardened turf found near volcanoes.

For the information of those who may be desirous of making mushrooms continually grow up from the *pietra fongaja*, and of increasing the quantity, it may be necessary to remark, that this effect will be produced, if, according to the experiment of M. Gleditsch, the mushroom-stones kept in cellars be moistened with water in which mushrooms have been washed\*.

**XIII.** *On the Volcanic Island thrown up in the Neighbourhood of Iceland. By Captain VON LÖWENÖRN, of the Danish Navy †.*

**I**N the spring of the year 1783, a volcanic island, thrown up in the neighbourhood of Iceland, excited no little attention. According to the account of the sea-captain, who first saw it, exactly at the time when it first arose, smoke and flame seemed to rise from the sea, but no land or island was to be seen. It needs excite no wonder, therefore, that the observer was thrown into the greatest consternation, as he says himself, when he beheld the sea on fire! He and the whole crew therefore concluded that the end of the world had arrived, and they all began to prepare themselves for the awful moment: but, as they heard no trumpet, and as the sun shone in the firmament with his usual brightness, after considering what the phenomenon might be, they at last concluded that Iceland had been swallowed up by an earthquake; that this was a remainder of it; and that the flames arose from Hecla, the well-known volcano of that island. Full of this idea, they were just on the point of re-

\* Does not this furnish a hint to those who rear mushrooms in gardens on beds of horse-dung? EDIT.

† From *Geographische Ephemeriden*, 1799.

turning, in order to convey intelligence of this event to Denmark; but very luckily they soon after discovered the coast of Iceland.

The place where this volcanic eruption was seen, lies only  $7\frac{3}{4}$  nautical miles, fifteen to a degree, from the south-west extremity of Iceland. Hitherto they had seen no land, but recognising Iceland, the ship reached the place of destination, and completed her voyage. Other ships, which arrived later, saw a small island from which the eruption had arisen; but it always exhibited, as might naturally be expected, a different appearance. The same year smoke and flames were seen to arise from the nearest part of the opposite coast of Iceland.

As there have been many instances of such eruptions from the sea producing islands, this event attracted the notice of government, and the year following orders were given to the ships bound to Iceland to examine the new island; but it had entirely disappeared, so that no traces of it were to be found. Towards the conclusion of the year, however, an unfortunate accident happened, which, without doubt, was occasioned by sunken rocks forming a part of the island which had disappeared.

A Danish ship of war of 64 guns, called the *Infødsretten*, was expected from the East Indies, and intelligence had already been received that she had sailed from the Cape of Good Hope; but after that period no farther account was heard of her till the year 1785, when some vessels returning from Iceland reported, that some fragments of this ship, together with the long-boat, had been driven ashore on the coast of that island. According to every account, and by comparing the different circumstances, it appears to me certain that the above ship was wrecked on these rocks, then no longer visible above the surface of the sea. It is impossible that such a large boat could have been conveyed from a ship without the hands of men, unless the ship had been dashed to pieces. This boat was not only driven to land entire, and in good condition, though without any person in it, but there was found in it a box filled with wax candles. At the distance of about a quarter of a mile from the boat there were

found various pieces of the same ship, which could be easily known by some distinguishing marks. These parts, of different forms and sizes, would not have been thrown on shore so near each other if the misfortune had happened at a greater distance; the billows, currents, &c. would certainly have driven them on shore at places more remote from each other. Besides, these fragments were driven on shore by a wind which blew in a direction from these rocks, and nothing else of this misfortune had been perceived on the coast.

From all these circumstances I conclude that this vessel had experienced a very bad voyage home from the Cape of Good Hope, for that year easterly winds were exceedingly prevalent in the northern seas. A great many ships, and particularly men of war, preferred going round Great Britain to the passage through the Channel. It is probable that the ship in question may have been in want of some necessary, such perhaps as fresh water. The captain, besides, was well acquainted in Iceland, where I myself was with him, some years ago, as lieutenant on board a ship which he then commanded, and on this account he probably intended to run into some of its harbours, but unfortunately struck on the sunken rocks, the remains of the volcanic island. In this distressed situation the crew, no doubt, had recourse to the only probable means left for saving their lives by hoisting out their long-boat, and while employed in this labour the ship, it is likely, went to pieces, and the people were lost, as none of them were ever seen or heard of.

During my expedition to Iceland in the year 1786, I made it my business to make some researches in regard to this volcanic island, though at that time no suspicion was entertained that the above ship had been wrecked in this place; for this conjecture was only a consequence drawn from my researches.

When I arrived in Iceland, where, on account of the business entrusted to my charge, as well as on account of the observations which I was obliged to make for the improvement of charts, I found it necessary to remain some time with my ship in Holmens-hafen, and had at my disposal a small vessel which was lying there, I ordered Lieutenant,

now

Now Captain Grove, to cruise about with this small vessel in the neighbourhood of the place where the volcanic island had been seen. He remained there some days, and though he often sounded with a line of more than a hundred fathoms, found no bottom, so that he lost all hope of making any discovery; but, just when he was about to return, he observed, contrary to all expectation, that the waves broke over some rocks lying exactly level with the surface of the water. As he now entertained no doubt that he had found what he had been sent in quest of, he took the bearings and distances from the nearest part of the coast of Iceland, and transmitted to me an account of his observations.

When the business of the expedition was ended, and I was about to return at the end of the summer, I resolved to visit this interesting point myself, and to ascertain its real position by actual observation. I took my departure, therefore, from some small islands, or rocks, which lie before Cape Reikianös, the south-west extremity of Iceland, and the outermost of which is called the Grenadier's Cap, distant  $3\frac{1}{4}$  miles south-west from the Cape. As the weather was exceedingly favourable, I was so fortunate as to obtain its latitude by the meridian altitude of the sun, and its longitude by a timekeeper. Though the timekeepers which I carried with me were not of the best kind, as I had quitted the same day one of the ports of Iceland, where I observed their rate of going, their relative errors could not be of great importance. I determined, therefore, the position of the rock called the Grenadier's Cap at  $63^{\circ} 43' 40''$  north latitude, and  $25^{\circ} 35' 40''$  west longitude from the meridian of Paris. This agreed pretty nearly with the observations of Verdun de la Crenne, Borda, and Pingré\*; especially as I have good reason to believe, that, from a want of sufficient knowledge of the coast of this country, they placed Cape Reikianös three minutes too far north, as they make the latitude to be  $63^{\circ} 55'$ . As I found also, by the most accurate observations that could be made at sea, that these dangerous rocks lie  $47'$  in a direction south-west from the true meridian, and just four

\* *Voyage fait par ordre du Roi en 1771, 1772.*

miles from the before-mentioned Grenadier's Cap, the positions of these rocks will be  $63^{\circ} 32' 45''$  north latitude, and  $20^{\circ} 2' 50''$  west longitude from the meridian of Paris.

As I now proceeded to get a sight of these rocks, Captain Grove, who was on board my ship, concluded, from his former observations, that we could be at no great distance from them, having now quite lost sight of the Icelandic coast; and the before-mentioned rocks, which lie to the south-west from Iceland, though the weather was clear, being now scarcely discernible. My companion, therefore, asked whether it was prudent to advance so straight upon it. While we were talking on this subject the people called out, and immediately every eye was directed to the spot, where we saw before us the waves breaking over a rock. We immediately put about ship, and heaved the lead, which was in readiness, and found the depth twenty-six, and soon after forty fathoms, but a little farther no ground was to be found with a line of a hundred fathoms. Some tallow had been put into the bottom of the lead, as usual, to enable us to determine the nature of the bottom by the substances which adhered to it. By these means we obtained small fragments of stone which were entirely lava, or of a volcanic nature. The rock is not large, and the water around it is exceedingly deep. Its height is exactly equal to that of the surface of the sea, or rather a little lower; and for that reason it cannot be seen till one approaches very near to it, or when the waves break over it.

The origin of the volcanic island, which was seen in this place in the year 1783, I explain in the following manner:—The rock which now remains formed the crater, which at that period threw up flames and smoke. The large quantity of lava which issued from it, being accumulated on the bottom of the sea around the crater, may at length have been raised above the surface of the sea, and even to a considerable height. But as this volcano lies in a part of the ocean where prodigious billows prevail, and roll over each other throughout a wide extent of sea, it is probable that such a structure would soon be destroyed by their violence, especially as there is a great depth of water around it, in which it might easily  
be

be overturned. It is known also that the same year a considerable quantity of pumice-stone and volcanic substances of the like kind, the specific gravity of which was lighter than that of water, was cast on shore in Iceland, and found floating on the sea by mariners.

Had the eruption taken place in a calmer sea, and the depth around it been less abrupt, the thrown up mass would have consolidated itself by its own weight, and would have in time become an island; of which we have had instances in the Archipelago, in the East Indies, and different parts of the ocean. Had it taken place on the continent, or in an island, it would have formed a mountain. It is not necessary that a volcano should always arise from a mountain: volcanoes have been seen to burst forth in plains; but the invariable consequence is, that the volcanic matter, by being accumulated, and, as it were, piled up, forms a mountain. Now, as the violence of the waves may have easily washed away the loose matter accumulated round the crater, there is no absurdity in supposing, that, as the billows rolled over the mouth of the crater, the fire was at length overcome by the water, and the volcano extinguished.

The crater, consisting of rock, has remained. It is well ascertained that a rock existed in this place before the eruption; and it is confirmed, by late observations, that it exists still. An obscure notion prevailed among the seamen who frequented Iceland, that there was a blind rock\* in this neighbourhood called *Fugle-Skiir* (Bird's rock). This name I have retained in my charts, though the existence of it is denied by many seamen, because they passed without seeing it. But, under such circumstances, the testimony of one who has seen it is of more weight than a hundred who deny its existence because they did not see it. This confirms me in the opinion that the crater had existed long before in the same state.

To conclude, it may not be superfluous to remark, in order to strengthen this opinion, that, nearly in the same direction from the south-west extremity of Iceland, as already mentioned, there are five small islands or rocks, the outer-

\* Rocks lying under the water, and which are therefore more dangerous, are by seamen called *blind rocks*.

most of which lies at the distance of  $3\frac{1}{2}$  miles from Cape Reikianös. Between these the water is deep; ships which go to, or come from, the west side of Iceland, commonly pass through them, when they first get sight of the land and rocks. By the Danish seamen they are called *Fugle-Skiör*, because they are frequented by a great number of sea-fowl; but by the inhabitants they are called *Fild Eyarne*, (fire islands.) May not this afford reason to conjecture that in former times they had volcanic eruptions? and the volcano which appeared in the year 1783 may probably have existed long before.

XIV. *Experiments on some peculiar Matters drawn from Animal Substances treated with the Nitric Acid.* By C. WELTER\*.

THE author having treated silk with the nitric acid, to obtain from it the oxalic acid, was surprised to find that at the end of his process he obtained a silky-looking salt of a golden-yellow colour, and which, on the approach of a piece of red-hot coal, exhibited all the effects of gunpowder. As he made the experiment only once, he thought it of importance to give a particular account of the process, in order that it might be repeated.

On one part of silk he poured six parts of nitric acid of the shops, adding a little concentrated nitric acid. After it had rested two days, he distilled this mixture. He then poured what had passed into the receiver, on what remained in the retort, and filtered the whole. The oxalic acid crystallising on the filtre, he put the whole again into the retort, and added a pretty large quantity of water, which had served to wash the filtre. He distilled off a part of the water; but as the residuum did not crystallise, returned, by elevating the receiver, what had passed over; and, after repeating this operation several times, obtained for residuum an acid liquor of the weight of the silk employed, and which contained small granulated crystals.

This liquor shewed no traces of the oxalic acid. It was yellowish, and communicated that colour to the fingers and

\* *Bulletin des Sciences*, Vol. II. No. 1.



to silk. The tint was not in the least weakened by washing. C. Welter saturated this liquor with lime; and having concentrated it, he added alcohol, which took up a matter of a gummy appearance. The alcohol, diluted with water, being evaporated, there remained a yellow substance mixed with solutions of the nitrat and muriat of lime. These salts were decomposed by carbonat of pot-ash, and the liquor, separated from the carbonat of lime, was subjected to evaporation. It gave golden-coloured crystals, which had the fineness of silk; and detonated like gunpowder, producing a black smoke. These crystals are soluble in water and alcohol, and crystallise on cooling. They are deprived of their colour by the oxygenated muriatic acid. The sulphuric acid disengages from them the odour of the nitric acid. The muriatic acid precipitates, from a solution of them, small micaceous, whitish, volatile crystals, which in the fire exhale a bitter and inflammable smoke.

This golden-yellow coloured detonating and crystallisable substance is by the author called *amer* (bitter); its crystals appear to be octaedral. As animal substances become yellow by the contact of the nitric acid, C. Welter endeavoured to extract *amer* from raw beef; but he found it combined with another substance, which, like it, could not be altered by the nitric acid. This combination, soluble in the concentrated nitric acid, is separated from it by water, under the form of a yellow powder, which does not lose its colour by exposure to the air, and which might perhaps be useful in painting.

What made C. Welter presume that this powder is composed of *amer* and another substance, is, that he obtained the latter by treating sponge with the nitric acid. It is colourless, soluble in concentrated nitric acid, and suffers itself to be precipitated by water like the preceding powder.

What has been here said seems to shew that animal matters treated with the nitric acid give as residuum two substances unalterable by that acid, and which are found either in the state of combination, or separate. It appears that silk gives pure *amer*, sponge gives the second substance pure, and beef a combination of both. The *amer* is yellow, and soluble in water: the combination of both is as insoluble in water as the substance obtained from sponge, but coloured.

*XV. Reflections on the Quality of Earthen-Ware, and the Results of the Analysis of some Earths and common Kinds of Earthen-Ware. By C. VAUQUELIN\*.*

**F**OUR things may occasion the difference in the qualities of earthen-ware: 1st, the nature or composition of the matter: 2d, the mode of preparation; 3d, the dimensions given to the vessels; 4th, the baking to which they are subjected. By composition of the matter, the author understands the nature and proportions of the elements of which it is formed. These elements, in the greater part of earthen-ware, either valuable or common, are silex, argil, lime, and sometimes a little oxyd of iron. Hence it is evident that it is not so much by the diversity of the elements that good earthen-ware differs from bad, as by the proportion in which they are united. Silex or quartz makes always two-thirds at least of earthen-ware; argil or pure clay from a fifth to a third; lime from 5 to 20 parts in the hundred; and iron from 0 to 12 or 15 parts in the hundred. Silex gives hardness, infusibility, and unalterability; argil makes the paste pliable, and renders it fit to be kneaded, moulded, and turned at pleasure. It possesses at the same time the property of being partially fused by the heat which unites its parts with those of the silex; but it must not be too abundant, as it would render the earthen-ware too fusible and too brittle to be used over the fire.

Hitherto it has not been proved by experience that lime is necessary in the composition of pottery: and if traces of it are constantly found in that substance, it is because it is always mixed with the other earths, from which the washings and other manipulations have not been able to separate it. When this earth, however, does not exceed five or six parts in a hundred, it appears that it is not hurtful to the quality of the pottery; but if more abundant, it renders it too fusible.

The oxyd of iron, besides the inconvenience of communicating a red or brown colour, according to the degree of

\* *Bulletin des Sciences*, Vol. II. No. 2.

baking, to the vessels in which it forms a part, has the property of rendering them fusible, and even in a greater degree than lime.

As some kinds of pottery are destined to melt very penetrating substances, such as salts, metallic oxyds, glass, &c. they require a fine kind of paste, which is obtained only by reducing the earths employed to very minute particles. Others destined for melting metals, and substances not very penetrating, and which must be able to support, without breaking, a sudden transition from great heat to great cold, require for their fabrication a mixture of calcined argil with raw argil. By these means you obtain pottery, the coarse paste of which resembles *breche*, or small-grained pudding-stone, and which can endure sudden changes of temperature.

The baking of pottery is also an object of great importance. The heat must be capable of expelling humidity, and agglutinating the parts which enter into the composition of the paste, but not strong enough to produce fusion; which, if too far advanced, gives to pottery a homogeneity that renders it brittle. The same effect takes place in regard to the fine pottery, because the very minute division given to the earths reduces them nearly to the same state as if this matter had been fused. This is the reason why porcelain strongly baked is more or less brittle, and cannot easily endure alternations of temperature. Hence coarse porcelain, in the composition of which a certain quantity of calcined argil is employed, porcelain retorts, crucibles, tubes, and common pottery, the paste of which is coarse, are much less brittle than dishes and saucers formed of the same substance, ground with more labour.

The general and respective dimensions of the different parts of vessels of earthen-ware have also considerable influence on their capability to stand the fire.

In some cases the glazing or covering, especially when too thick, and of a nature different from the body of the pottery, also renders them liable to break. Thus, in making some kinds of pottery, it is always essential, 1st, to follow the best proportion in the principles; 2d, to give to the particles of

the paste, by grinding, a minuteness suited to the purpose for which it is intended, and to all the parts the same dimensions as far as possible; 3d, to carry the baking to the highest degree that the matter can bear without being fused; 4th, to apply the glazing in thin layers, the fusibility of which ought to approach as near as possible to that of the matter, in order that it may be more intimately united.

C. Vauquelin, being persuaded that the quality of good pottery depends chiefly on using proper proportions of the earthy matters, thought it might be of importance, to those engaged in this branch of manufacture, to make known the analysis of different natural clays employed for this purpose, and of pottery produced by some of them, in order that, when a new earth is discovered, it may be known by a simple analysis whether it will be proper for the same object, and to what kind of pottery already known it bears the greatest resemblance.

	Heffian Crucibles.	Argil of Dreux.	Porcelain Capsules.	Wedgewood's Pyrometers.
Silex - - -	69	43.5	61	64.2
Argil - - -	21.5	33.2	28	25
Lime - - -	1	3.5	6	6
Oxyd of iron -	8	1	0.5	0.2
Water - - -	-	18	-	6.2

Raw kaolin 100 parts.—Silex 74, argil 16.5, lime 2, water 7. A hundred parts of this earth gave eight of alum, after being treated with the sulphuric acid.

Washed kaolin 100 parts.—Silex 55, argil 27, lime 2, iron 0.5, water 14. This kaolin, treated with the sulphuric acid, gave about 45 or 50 per cent. of alum.

Petuntzé.—Silex 74, argil 14.5, lime 5.5, loss 6. A hundred parts of this substance, treated with the sulphuric acid, gave seven or eight parts of alum. But this quantity does not equal the loss sustained.

Porcelain of retorts.—Silex 64, argil 28.8, lime 4.55, iron 0.50, loss 2.77. Treated with the sulphuric acid, this porcelain gave no alum.

XVI. *Eleventh Communication from Dr. THORNTON, Physician to the Mary-le-bone General Dispensary, &c. &c. &c. relative to Pneumatic Medicine.*

A REMARKABLE CURE OF AN ULCER OF THE LEG.

MR. RODERICK M'KENNON, aged 67, went in the year 1758 as Assistant Apothecary to St. George's Hospital, where he had his washing, board and lodging found him, with a suitable salary. In June 1795, whilst in this employ, he went to see Dr. M'Nab, who then resided in Great Suffolk-street; and as he was at the door, a bitch in the house, who had puppies, furiously flew at him, and seized him near the calf of the leg, making a deep lacerated wound. The wound soon after became dreadfully inflamed, poultices were applied, and it was near a fortnight before he made his case known to the surgeons of the hospital. He was now confined to his room, and these most experienced and eminent practitioners continued their humane attentions to him above a twelvemonth, trying a variety of different applications, until, finding his case baffle all their endeavours, he was dismissed his employ, and left the hospital as incurable. Added to this dreadful and unforeseen affliction, he had an asthma, which had existed on him above ten years, and was obliged frequently to sit up the greatest part of the night with the windows wide open to procure breath. He was now in the vale of years, and with a gloomy prospect before him; for no salary was allowed this almost superannuated servant of a public charity, to which he had been attached above thirty years; and he had a wife and daughter to provide for. After quitting the hospital, Mr. Carpue, a surgeon no less distinguished for zeal than abilities, for some months attended him; but finding all his endeavours ineffectual, he reluctantly took his leave of him as incurable. Such was the deplorable state of this unfortunate sufferer, when Mr. Carpue recommended him for the trial of the oxygen air, using these very expressions: "Poor Roderick has been under Mr. Home's care," (an eminent surgeon, brother-in-law to John Hunter,) "in

St. George's Hospital, which he left as incurable, and since under my care for several months; and so bad is his case, that I am sure if you can cure him you can cure the devil.\* Being no surgeon, I could have no wish to accept of such a case but for the cause of humanity and the sake of science; and I feel extreme delight in saying, that poor Roderick is now perfectly cured, the ulcer is healed, his asthma gone, and, in order that the philosophic world may see fuller particulars respecting this extraordinary cure, I am happy to be able to add the following testimonies:

*A Letter to Dr. Thornton, from Mr. Carpus, Surgeon at the York-Hospital.*

DEAR SIR,

I have seen Mr. McKennon, and have examined his leg, and think the cure you have wrought on it is indeed very astonishing. When he first came under my care, he laboured under an immense ulceration, extending from the external angle of the right leg, which reached as high as the junction of the tendons of the gastrocnemii and solæus muscles. At this period the tendons of the peronæal muscles had fluffed, and in consequence I applied charcoal, which produced very considerable good; but upon mentioning this to a friend, who knew the case well\*, he said, "It was immaterial what remedy I used, for it was a case in which he was certain nothing would prove effectual." After this I applied the diluted nitrous acid, and seemingly with advantage; but being obliged to go into the country, I left him under the care of another surgeon; and when I saw him, after an absence of six weeks, I found the sore in a very unfavourable condition; and for four months I used different applications, but to no purpose, and I conceived the case now to be perfectly incurable, and as such mentioned it to you when I had the pleasure of meeting you at Mr. Heavyside's: and I then proposed him to you for the trial of the oxygen air, as his case, if successful, would prove most decidedly its efficacy; for, in the multitude of sore legs I have at-

\* We believe this to be Dr. Bailey, physician to St. George's Hospital.

tended, I must acknowledge I never saw a worse case, old West-India sores excepted.

I have the honour to be, Sir,

Your obedient servant,

J. C. CARPUE.

Having referred Mr. M<sup>c</sup>Kennon to Mr. Spencer, a surgeon in Charlotte-street, Fitzroy-square, who administers the pneumatic remedies, I received the following letter :

*A Letter to Dr. Thornton from Mr. Spencer.*

SIR,

I here enclose the treatment and the progressive cure of the ulcer which occupied the external angle of the right leg of Mr. M<sup>c</sup>Kennon. For six weeks, by your direction, he daily took a gallon of oxygen air, mixed with four times that quantity of atmospheric air. The ulcer discharged properly, but seemed to heal very slowly : in consequence I gave him a double dose, and after a fortnight it produced very feverish symptoms, when he took by your order some purgatives, and then he resumed his usual dose of superoxygenated air daily, until the ulcer, diminishing by degrees, was at last completely healed, there being no discharge, the whole cicatrising, and the new-formed surface looking extremely healthy. During this period no particular application or dressings were made use of by me, nor any medicine directed by you, but what before he said he had taken gallons of ; so that I attribute his extraordinary cure entirely to the efficacy of the oxygen air. Happy in being able to give my testimony to so remarkable a case, I have the honour to be, dear Sir,

With the profoundest respect,

Your obedient humble servant,

T. SPENCER.

REMARKS BY DR. THORNTON.

As Mr. M<sup>c</sup>Kennon took bark, some of the Faculty may not be willing to give to the oxygen air the merit in this cure ; I will therefore endeavour to state shortly my reasons for attributing every thing to this new remedy.

1. The

1. The operation of bark had been before tried; he had taken, he said, gallons of it.

2. When I first saw him, the fore, and muscles surrounding it, were wholly insensible; he did not feel a needle piercing them, nor could he perceive even the corrosive operation of caustic.

3. After inhaling the vital air but a few days, sensibility was restored, as both Mr. M'Kennon and Mr. Carpue witnessed.

4. Having cleansed the wound, it would remain dry; but even whilst inhaling the vital air, the whole surface was immediately covered with a fine dew, as Dr. Monro and others witnessed.

5. To shew the progress of amendment whilst inhaling the superoxygenated air, I am happy to be able to lay before the philosophic world the following testimony of an impartial observer, Dr. Douglas of Baliol-College, Oxford:

“Towards the middle of March 1798, I first saw Mr. M'Kennon. He had then a large and very foul ulcer, extending some inches above the right ancle. From that time to the present (April 30) I have repeatedly seen him, and *each time could not possibly fail to be sensible of a most manifest improvement.* At present *the ulcer is diminished at least one-half in size since I first saw him*; the edges have a fine healthy appearance, and its general surface is astonishingly altered for the better.”

6. When the oxygen air was left off, the fore remained stationary, and visibly improved when he again resumed it.

7. My strongest argument, however, is the success in this and in other cases equally desperate.

Mr. Munt had been before cured of a fore leg of eighteen years standing.

Mr. Atwood was cured of a fore leg of two years. When I asked Mr. Cruikshank whether it was true he had condemned the leg? he answered, with his usual emphasis, “I not only condemned his leg, but his life; for he was of so watshy a constitution that he could not have lost the one without the other.”

Next the cure of Patterson. When I sent him to shew



his leg to Mr. Cruikshank, which still possesses the marks of numerous ulcers, seeing varicose veins, this experienced surgeon said: "Tell Dr. Thornton that he is mistaken if he supposes he has made a permanent cure; for varicose ulcers were never cured without an operation, which, if he wishes, I will perform." The man, frightened at first, and then astonished, replied: "Sir, I have been cured perfectly now these three years."—"That alters the case," answers this distinguished anatomist; "then tell Dr. Thornton that he has performed a most wonderful cure."—Patterson still continues well; nor does there seem the smallest cause to suspect a relapse.

The cure of Mr. Wilkinson\*, who had a sore leg twelve years, is not less extraordinary. In this case I observed a peculiar phenomenon, alone explicable by the operation of the oxygen air. The fingers of both hands at their ends looked very red, as red as raw meat, were swollen, and felt very painful. The same was mentioned to me in private conversation by Dr. Beddoes in a patient of his, who, finding an asthma relieved by a small dose of vital air, took as much as he could at one time, produced a fever, and this same phenomenon I have just mentioned above.

This leads me to repeat an observation I have before often expressed, that oxygen air promises to be an useful remedy in sore legs; for why have we not sore arms? The nearness of this part to the heart seems to be the only philosophic reason; and therefore a direct powerful stimulus to the heart, as oxygen air, promises the most certain good, aided by the invigorating effects of bark, steel, and other tonic medicines; not but that I would advise, where it can be properly done, as in hospitals, trials to be made with the vital air without medicine, to prevent all cavil; although it is undoubtedly unimportant to the sufferer by what means he is treated, so that he is *but cured*; and, until the contrary is proved, I shall ever think, that medicines, judiciously employed, certainly cannot *impede* the operation of oxygen, but may *assist*.

I now take my leave of the philosophic world, not from dearth of materials, for I have cures by me yet more im-

\* The cases of Mr. Munt, the Rev. Mr. Atwood, Patterson, and Mr. Wilkinson, are related in Dr. Beddoes's *Considerations on Factitious Airs*.  
portant

portant than those before related by me, that deserve, I think, to be recorded; and daily experience, in a large public charity\*, where the airs are administered under my direction, would afford me other frequent opportunities; but according to my promise, mentioned in my first communication, I was to continue writing on this subject only until the establishment of the Pneumatic Institution under Dr. Beddoes, which I am happy to announce, and that shortly the public may expect a periodical quarterly publication, relating chiefly to this important investigation, from the masterly pen of that eminent physician.

## INTELLIGENCE,

AND

## MISCELLANEOUS ARTICLES.

### LEARNED SOCIETIES.

#### GERMANY.

THE Electoral Academy of Sciences at Manheim has proposed the following question as the subject of a prize for the year 1801:—

“Are the azotic (*flück*) gases, which are produced from so many totally dissimilar substances, and in ways so different, exactly the same in all their chemical properties and bases (simple azotic gases,) with that of the atmosphere? and has the nitrous acid the same azote for its acidifying base, as the atmospheric azotic gas?

“The partizans of the antiphlogistic doctrine seem to admit both, but without satisfactory proof. 1, Since, for want of sufficiently accurate examination, they admit in each of these gases all the known properties of azotic gas, because they destroy animal life, extinguish flame, and manifest no acid properties: but, 2, after all their analytic and

\* The Mary-le-bone General Dispensary.

synthetic proofs, they have still left well-founded doubts, (a) whether the electric spark, in its passage through oxygen or azotic gas, does not itself undergo a chemical decomposition, and furnish the basis of the nitrous acid; (b) and how, by the same means, (a red heat, and the electric spark,) the nitrous acid is decomposed into oxygen and azotic gases, and can be again recomposed from them; and, (c), since oxygen and azotic gases have so great an affinity for each other in the atmosphere, why, when the former is added in a sufficient quantity, imperfect nitrous acid is not immediately produced, as is the case when oxygen gas is added to the azote in nitrous gas, by which perfect nitrous acid is immediately produced?

“The papers on this subject, written either in Latin or French, must be transmitted before the 1st of November 1800, to M. I. Kency, secretary to the academy, with the name of the author, in a sealed note, and any motto chosen at pleasure. The prize is a gold medal, of the value of 50 ducats.”

#### HOLLAND.

The Dutch Society of the Sciences at Haerlem proposed in 1793, and afterwards in 1796, the following question: “What light has Lavoisier’s system, and his method of examining organic substances, furnished towards a more accurate knowledge of the human body?” But, as no satisfactory answer was received, they have proposed it again in the following manner:—

I. As a great number of new discoveries have been made since the question was first proposed, and as the circumstances of it have thereby acquired more extent than to admit of their being properly comprehended in a single treatise, the Society have resolved to divide the different objects of it into three new questions for the present year, and to fix the period of receiving answers at the 1st of November 1800.

1. What light has the new chemistry thrown on the physiology of the human body?

2. How far has the light, thrown on the physiology of the human body, contributed to a better knowledge than before of the nature and causes of certain diseases; and what useful

consequences, more or less confirmed by experience, can be deduced from it in regard to the practice of medicine?

3. How far has the new chemistry contributed to afford an accurate idea of the mode of action of different internal and external medicines, which have been long used, or only lately recommended? And, what advantages can arise from a more accurate knowledge of this point in regard to the treatment of certain diseases?

As some learned men have introduced hypotheses built on too weak a foundation, in regard to the application of the principles of the new chemistry to physiology, pathology, and therapeutics; and as this is highly prejudicial to the progress of these sciences, to which the new chemistry, however, promises so much light, if, according to Lavoisier's rule, we admit nothing in chemistry, or the employment of chemical principles, but what is founded on decisive experiments, the Society requires, that those who are inclined to answer these questions will make an accurate distinction between what is proved, and what is merely hypothetical; and that, in regard to hypotheses, the candidates will confine themselves to a bare mention of them, and of the few grounds on which they rest; because the principal point which the Society wishes to obtain is, that those who follow the medical or surgical profession in Holland, and who are not yet sufficiently acquainted with the progress of the new chemistry, and its application, on well-founded principles, to physiology, pathology and therapeutics, will procure such works as may be best calculated to inform them what light the new chemistry has actually thrown on these sciences; and what facts are founded on too weak grounds; and what have been too rashly adopted, or are still too doubtful to be depended on. Each of these papers will be examined separately: those, therefore, who wish to answer more than one question, must send a paper for each.

II. The Society requires a plan, capable of being carried into execution, for rendering productive the large uncultivated districts of the republic, particularly in Guelderland, Overijssel, Drenthe, and Dutch Brabant.

III. The

III. The following prize questions are again proposed:—

1. A natural history of the whale; in order to furnish hints for its being more easily discovered and caught, and afterwards converted to use. This question to be answered before the 1st of November 1802.

2. What has experience taught in regard to the use of certain animals which in the Netherlands appear to be hurtful; and what means are to be employed for extirpating them? For this question no period is defined.

3. What indigenous plants, the virtues of which have been hitherto unknown, might be employed in the apothecaries shops in Holland to supply the place of foreign medicines? The virtues of them must be established, not by foreign testimony, but by the testimony of natives of the country.—The time for answering this question is indefinite.

4. What indigenous plants, not yet employed, might be introduced into use as good and cheap food? And, what foreign nutritive plants might be cultivated for the same purpose?—No definite time.

#### FOSSIL WOOD FOUND AT A GREAT HEIGHT.

In a paper lately read before the French National Institute, it appears that C. Villars, Professor of Natural History, of Grenoble, saw, near a glacier in the department of Isere, some fossil wood buried among turf at the height of 2320 metres above the level of the sea, and 850 metres above the most elevated line at which wood grows at present. The mountain on which this interesting discovery was made, is that of Lans, in the canton of Oisans. The trees found there are, mountain-ash, birch, and the common larch. The roots and part of the trunks can be plainly distinguished. The last of these trees does not grow at present in the neighbourhood.—The author of this memoir ascribes the greater degree of cold, which now prevails on these mountains, to two principal causes: first, the valleys becoming deeper, which has changed the elevation of the summits in regard to their bases and the surrounding countries: the second is the destruction of the ancient forests, which had gradually extended themselves to great heights, but which, when once destroyed, cannot grow

up again at the same heights, because the trees are deprived of that mutual shelter which they afforded to each other.

#### NEW THEORY OF RESPIRATION.

Professor Herholdt read lately, before the Academy of Sciences at Copenhagen, a memoir respecting some experiments made by him and M. Rafn on living animals, in order to discover the mechanism of respiration; having in view, at the same time, the cure of wounds in the breast. The professor shewed that the best authors on surgery have hitherto explained the mechanism of respiration in a manner diametrically opposite to what it really is; so that, by applying their theory to the cure of wounds in the breast, they have followed a method altogether false. According to his experiments the lungs have not, as has been maintained, an expansive force peculiar to them, but the movement is performed by the action of the diaphragm, to which sufficient attention has not hitherto been paid. When there are wounds in the breast, the atmospheric air enters by them on inspiring into the cavities of the thorax, and issues on expiring. This has been proved by experiments made on horses, dogs and cats.

M. Herholdt and Rafn, in examining the manner in which the frog breathes, remarked, that this animal is without a diaphragm, and that its lungs at the same time have no expansive force; but that a small membrane, by means of which it can shut its mouth hermetically, discharges the function of the diaphragm; so that, when it is prevented from shutting its mouth by inserting into it a small rod, the animal dies in a few minutes, because it is no longer able to breathe. When it is suffered to shut its mouth before it is entirely dead, or when it is only in a state of asphyxia, it soon recovers. If a frog be deprived of this membrane, by cutting it entirely off, or only in part, so that its mouth can no longer be hermetically shut, it expires in a longer or shorter time according to the size of the aperture made: on the first view it appears very paradoxical that man, as well as the greater part of animals, loses his life by not being able to breathe when his mouth and nose are shut, and that the frog dies because it cannot breathe when its mouth is opened. The explanation  
of

of this phenomenon, however, is easy when we recollect that the lungs have no expansive force. In consequence of this new theory, M. Herholdt has succeeded in curing very dangerous wounds made in the breasts of dogs.

The above experiments, which were communicated to the Philomatic Society at Paris, by M. Manthey, were repeated with success, by the commissioners of that Society, on frogs and salamanders. If a gag be put into the mouth of one of these animals, so as to prevent it from shutting its mouth, it dies at the end of half an hour. The respiration is performed as follows:—The mouth being absolutely shut, the frog dilates its throat, and the air rushes in by the nostrils; it afterwards contracts its throat, and the air penetrates to the lungs, because, no doubt, there is a small valve in the nostrils which prevents it from escaping by the same way that it entered; for the membrane, which the Danish authors assert they observed in the mouth, could not be seen by the commissioners. Lizards and serpents, which have ribs, breathe like other animals; and a forced opening of the mouth does not kill them.

#### MEDICAL PNEUMATIC INSTITUTION.

All who wish well to the interests of humanity will rejoice to learn that this institution, as useful as novel, is at last so far established that there is every reasonable ground to believe the objects of it will now receive that ample and fair investigation their importance demands. The learned, ingenious, and meritoriously persevering founder of it, has already published a *Notice of Observations made at the Institution*, which contains some highly interesting remarks on the effects of a gas not before applied to medicine, but which promises, in skilful hands, to be one of the safest and most powerful agents hitherto discovered.

Mr. Davy, superintendant of the institution, than whom it would hardly be possible to find one better qualified for the situation, having made some experiments on *dephlogisticated nitrous gas*, so named by its discoverer Dr. Priestley, which proved that its composition, properties, and mode of action, had been mistaken by the latest experimenters, was induced

to inhale it. Dr. Beddoes relates the circumstances in the following words :

“ The first inspirations of the gas produced giddiness, fullness of the head, and, in short, feelings resembling those of incipient intoxication, but unaccompanied by pleasurable sensation. At this next experiment I was present. The quantity was larger, and the gas more pure. The scene exhibited was the most extraordinary I had ever witnessed, except in the case of that epileptic patient, whom I have described (*Considerations on Airs*, part iv. p. 13.) as agitated, in consequence of the respiration of oxygen gas, with a long succession of the most violent movements. The two spectacles differed, indeed, essentially in one respect. In the former every thing was alarming: in the latter, after the first moments of surprize, it was impossible not to recognize the expressions of the most ecstatic pleasure. I find it entirely out of my power to paint the appearances, such as they exhibited themselves to me. I saw and heard shouting, leaping, running, and other gestures, which may be supposed to be exhibited by a person who gives full loose to feelings excited by a piece of joyful and unlooked-for news. As in the case of the epileptic patient, *no weariness or depression followed*; so, in this case, *no exhaustion or languor or uneasy feeling took place*. The experiment Mr. Davy has very frequently repeated, and generally with the highest pleasurable sensations; and, except under particular circumstances, with considerable muscular exertions, which have not in any instance been succeeded by fatigue or sadness.”

A number of persons afterwards inhaled the same gas: the following extracts will convey some idea of the very singular effects produced by it:

“ Mr. J. W. Tobin (after the first imperfect trials), when the air was pure, experienced sometimes sublime emotions, with tranquil gestures; sometimes violent muscular action, with sensations indescribably exquisite; no subsequent debility—no exhaustion.—His trials have been very numerous. Of late he has felt only sedate pleasure. In Mr. Davy the effect is not diminished,

Mrs.



“ Mrs. Beddoes—Pretty uniform pleasurable sensations—propensity to muscular exertion, could walk much better up Clifton Hill—has frequently seemed to be ascending like a balloon, a feeling which Mr. Burnet strongly expressed.

“ Mr. James Thomson. Involuntary laughter—thrilling in his toes and fingers—exquisite sensations of pleasure—a pain in the back and knees, occasioned by fatigue the day before, recurred a few minutes afterwards. A similar observation we think we have made on others; and we impute it to the undoubted power of the gas to increase the sensibility, or nervous power, beyond any other agent, and probably in a peculiar manner.

“ Mr. Stephen Hammick, Surgeon of the Royal Hospital, Plymouth. In a small dose, yawning and languor.—It should be observed, that the first sensation has often been disagreeable, as giddiness; and a few persons, previously apprehensive, have left off inhaling as soon as they felt this.—Two larger doses produced a glow, unrestrainable tendency to muscular action, high spirits, and more vivid ideas.

“ Mr. Robert Southey, for many hours after the experiment, imagined that his taste and smell were more acute, and is certain that he felt unusually strong and cheerful. In a second experiment, he felt pleasure still superior—and has since poetically remarked, that he supposes the atmosphere of the highest of all possible heavens to be composed of this gas.

“ Robert Kinglake, M.D. Additional freedom and power of respiration, succeeded by an almost delirious but highly pleasurable sensation in the head, which became universal, with increased tone of the muscles. At last an intoxicating placidity absorbed for five minutes all voluntary power, and left a cheerfulness and alacrity for several hours. A second stronger dose produced a perfect *trance* for about a minute; then a glow pervaded the system. The permanent effects were, an invigorated feeling of vital power and improved spirits. By both trials, particularly by the former, old rheumatic feelings seemed to be revived for the moment.

“ Mr. Wedgwood, after breathing some time, threw the bag from him, kept breathing on laboriously with an open mouth, holding

holding his nose with his left hand\* without power to take it away, though aware of the ludicrousness of his situation—all his muscles seemed to be thrown into vibratory motion—he had a violent inclination to make antic gestures—seemed lighter than the atmosphere, and as if about to mount. Before the experiment, he was a good deal fatigued after a very long ride, of which he permanently lost all sense. In a second experiment nearly the same effects, but with less pleasure. In a third, much greater pleasure.”

The cases in which its effects were prejudicial are also stated with great candour. These were of the hysterical kind; but the consideration of the whole of these phenomena led to a happy application of the gas to the cure of palsy, and of diseases proceeding from a defect of nervous energy; and several instances of success are stated, which are well deserving of attention.

“We desire further,” says the author, “to try the effects of our new agent in palsy, and in the various cases of *true* deficiency of nervous power, which we have well learned to distinguish from cases of relative defect of irritable power. As the limits of the efficacy of every remedy ought to be determined, we shall not shrink from any case, by reason of its inveteracy. We intend to oppose our Nepenthe to the equable decay induced by time and intemperance; and we hope to palliate some of the evils of extreme old age itself.

“We are emboldened by experience to pledge ourselves for the safe employment of the gas. We shall, indeed, be sadly disappointed if it do not sometimes prove the most delicious of luxuries, as well as the most salutary of remedies. In saying this, it may be allowed me to suggest to those, who have not attended to the tenor of my opinions, that I now for the first time venture to hold forth these hopes. However urgently I may have recommended the investigation, my language, with regard to its issue, has always been, that *I would not answer for the discovery of a gaseous remedy in any denomination whatever of disease. That natural or forced decay may be repaired, and the faculty of pleasurable sensation renovated,*

\* This was practised in all the experiments.

is now no longer a mere conjecture supported by loose analogies—we see the strongest probabilities daily accumulating in favour of the opinion: It must only be remembered, that so desirable a change cannot be effected by the agent applied in any manner to any constitution. It must be properly used in proper cases.

“Considering the present abundance of expert chemists, we cannot presume that others will not be able to prepare the gas perfectly without our instructions. Nevertheless, those who attempt to use it medicinally should be apprized that the utmost care is necessary in its preparation and employment. A deleterious, instead of a salutary fluid, as the author can attest from his own painful experience, may easily be obtained. Probably neither Dr. Priestley, nor the Dutch chemists, ever procured that which can be respired with safety. The difference, and its causes, will hereafter be pointed out.

“If, therefore, in consequence of that promptitude which journals afford, experiments tending to invalidate any part of the preceding statement should be published, we hope that its perfect fidelity will not on that account be doubted. The experimenter will be found to have employed a different agent from ours. This we have no hesitation in declaring that we shall be able to shew.”

From the preceding short extracts, our readers will see what an interesting field is opened for the advancement of physiology and medicine; and we hope that some will be induced to assist an institution, which ought not to be confounded with ordinary charitable foundations. It is established for the sake of relieving from a species of distress, from which no degree of affluence can exempt—namely, that which arises from the imperfect state of medicine. Subscriptions are received by Mr. Coutts, Banker, Strand, London; and by Mr. Savery, Banker, Bristol.

#### CHROMAT OF IRON.

C. Pontier, correspondent to the *Journal des Mines*, transmitted lately to the Cabinet of the House of Instruction, among other interesting minerals, a substance in an irregular mass, of a dark brown colour, having a metallic splendour

and mean hardness, the specific gravity of which was found to be 4.0326. C. Pontier found it in the department of Var, à la Bastide de la Carrade, near Gassin, and considered it as the brown blende, to which indeed it has a considerable degree of resemblance, except that its specific gravity is far greater. This substance being analysed in the laboratory of the mines by G. Tassaert, was found to be chromate of iron, that is to say, a metallic salt formed by the combination of iron with the acid arising from the new metal discovered by C. Vauquelin, to which he gave the name of chrome. It seemed to contain, in 100 parts, 63.6 of that acid, 36 of iron; loss, 1.4. Chemists may now flatter themselves that chrome, which hitherto has been found only in the red lead of Siberia, in the ruby and the emerald, may be obtained in sufficient abundance to enable them to subject it to new researches.

C. Vauquelin and Tassaert, by continuing their experiments on this substance, have ascertained the following facts:

1. It does not melt alone by the blow-pipe, but with borax, to which it communicates a green colour like that of the emerald.

2. It is soluble in the muriatic acid, but slowly, and in small quantity. From its solution, which is of a greenish blue colour, it is precipitated white by alkalies.

3. It is soluble in oxygenated muriatic acid. From this solution, almost colourless, it is precipitated of a reddish brown by alkalies, and of an orange yellow by nitrate of lead.

4. It is not decomposable by potash, or the carbonate of potash, without the assistance of a degree of heat which carries it to incandescence.

5. It may easily be reduced by the known means. It then gives an alloyed mass, having only an external metallic aspect, easily fused with borax; brittle, but hard as steel.

6. This mass is exceedingly difficult to be decomposed. It must be treated successively and several times with potash, which combines with the chromic acid and dissolves it, and with the muriatic acid, which dissolves the oxyd of iron.

7. Other

7. Other experiments have proved the presence in this mineral of silice and alumine; so that C. Vauquelin and Tassaert believe it to be composed of the following substances :

Chromic acid	-	-	43
Oxyd of iron	-	-	34.7
Argil	-	-	20.3
Silex	-	-	2

They are of opinion also, that, the chromic acid being in sufficient quantity to saturate the oxyd of iron, this mineral is a triple combination of the chromic acid, the oxyd of iron, and argil.

8. The oxyds of chrome, or the chromic acid, may be employed in the porcelain manufacture. When pure, they give an emerald green, more beautiful than that of copper; and, mixed with lead or antimony, a canary-bird green. They may be employed also in painting, by separating the acid from the iron, and combining it afterwards with different metallic oxyds by double affinities.

#### METHOD OF CHECKING DECAY IN TREES.

The chestnut lives a long time, and often attains to an extraordinary size; but unfortunately the texture of its wood alters under certain circumstances: it becomes soft, falls into dust, a cavity is gradually formed in the heart of the tree, and this cavity, by the progress of the decomposition, becomes still larger; so that, at last, the trunk seems to consist of nothing but bark; and being too weak to support the branches, and resist the violence of hurricanes, it cannot long exist. It is by similar changes and decompositions of the ligneous principle, that trees, which have been growing for ages, are seen to perish in a very little time.

C. Chaptal, in travelling through different parts of France, and particularly the Cevennes and the department of Allier, observed, that a great number of chestnut-trees were hollow, and charred over the whole internal surface. He was told by the inhabitants of the country, that this process was employed to stop the progress of the canies, which otherwise would destroy the whole tree. When they observe that this

disease, which is very common and exceedingly fatal to the chestnut-tree, begins to make any progress, and to excavate the trunk, they collect heath and other vegetables, and set fire to them in the cavity, till the whole surface is completely charred. It happens very rarely that the tree perishes by this operation, and it is always found that this remedy suspends the effect of the caries. It is practised with the same success on the white oak. By comparing the effects of the cautery on the human body, in analogous cases of degeneration, we perceive a new similarity between diseases which affect the living organised beings of the two kingdoms, and between the remedies by which they may be checked.

#### ON THE GRAVITY OF TUNGSTEN.

The difficulty of bringing tungsten to complete fusion has hitherto prevented the specific gravity of this new metal from being with certainty determined. Some fixed it at 17.6, according to the experiments of the brothers d'Elhuyar; but many could not believe it to be so considerable. C. Guyton lately obtained a well-formed button, of the weight of 35 grammes, in a three-blast-furnace, where the intensity of the fire may be carried to about  $185^{\circ}$  of Wedgewood: but this button having broken by the pressure of the vice, into which it was put in order to be sawn, there was discovered at its centre a part not agglutinated, which, by exposure to the air, speedily assumed a purple colour, similar to that which the best fused tungsten, of such a degree of hardness as to render a file brilliant, exhibits on its surface. It results, from these experiments, that the specific gravity of the fused portion, separated from that which was only fused imperfectly, was 8.3406; that the infusibility and brittleness of this metal leave no other hopes of applying it to the arts, though there are abundant mines of it in France, but by mixing it with other metals, or by the property discovered in its oxyds of yielding fixed colours, and fixing vegetable colours.

#### FLUAT OF ARGIL.

C. Vauquelin has received from Denmark a white lamellated mineral brought from Greenland, which proves to be

fluat of argil, an earthy neutral salt never before found in a natural state.

#### ASTRONOMY.

C. Lalande has lately presented to the National Institute an account of his observation of the last opposition of Mars, with the calculations on that subject; and by comparing it with that of 1790, he has found that only 58 seconds are to be deducted from the place of the aphelion of Mars, employed in the last edition of his *Astronomy*. He announces a large work on this planet by C. Lefrançois-Lalande, his nephew.

C. Lalande has also given the calculation of eclipses of the sun or stars observed for some years, to deduce from them the position of different cities. He has found Hamburgh to be 30' 9" from Paris; Cobourg 34' 30"; Mulheim 21' 20"; Halle 38' 28"; and Konigsberg 1<sup>h</sup> 12' 35".

#### MONUMENT TO LINNEUS.

A monument has lately been erected in the cathedral of Upsal, to the memory of the great Linneus. It consists entirely of the porphyry of Elfvedal; is properly a pedestal in the form of an altar, the steps of which are of the brown stone of Oeland; and supports a medallion containing a bust of Linneus. The following is the inscription:

CAROLO A LINNE,  
BOTANICORUM PRINCIPI,  
AMICI ET DISCIPULI.  
M,DCC,XCVIIII.

#### SUBSTITUTES FOR COFFEE.

Count von Burgsdorf has lately laid before the Royal Academy of Sciences at Berlin, samples of coffee prepared from the solid parts of the beet-root, after the saccharine juice has been expressed.

After the most careful examination of all the substitutes for coffee hitherto employed, the Academy of Sciences at Petersburg has made known that the acorn is the best, as it possesses, when proper means are used to communicate to it the oily properties, all the requisite qualities of coffee. To communicate these oily properties, the following process is recommended:

commended : When the acorns have been toasted brown, add fresh butter, in small pieces, to them while hot in the ladle, and stir them with care, or cover the ladle, and shake it in order that the whole may be well mixed. By these means you will obtain the best and most harmless substitute for coffee hitherto employed.

#### SUBSTITUTE FOR TINCTURE OF BARK.

Dr. G. F. C. Fuchs, teacher extraordinary of medicine at Jena, has prepared from the ripe fruit of the horse chestnut, *Aesculus hippocastanum* LINN. when divested of the husks, an extract, which, according to his experiments, may be used, perhaps, instead of the expensive *extractum chinæ*, since the bark of this tree has been long known as a substitute for cinchona.

#### SUBSTITUTE FOR GALLS.

A German apothecary, named Trömer, has lately discovered, that the excrescences or knots on the roots of young oaks may be used as a substitute for galls. These excrescences are produced, in the same manner as the galls, by an insect, which, after pricking them, deposits its eggs in them. With vitriol of iron, in the same proportion as galls, they give a beautiful black ink, and may be used also in dyeing. In the spring, these excrescences may be found in great numbers on the small roots of the oaks, particularly on the south side, often about a hand's-breadth below the earth. Those found in summer have, for the most part, small apertures, capable of admitting a moderate-sized needle, but they are no longer filled with eggs. At this period they are more woody, and not so good; and therefore they ought to be collected in the spring.

#### TRAVELS IN AFRICA.

Mr. Horneman, whom we some time ago announced to have set out with the view of exploring the interior of Africa, has written a letter to Sir Joseph Banks, from Tripoli. He had travelled from Cairo, in Egypt, through the Lybian Desert, to Fezzan, the largest Oasis in the Great Sahara; a route hitherto unexplored by any European, whose travels have



have been communicated to the public. In the journey from Cairo to Fezzan, he halted at Sewah, which, from the notices of Mr. Brown, some months ago, had been clearly ascertained to be the Oasis of Ammon.

Mr. Horneman's new observations, made at his leisure on the spot, now place this matter beyond all manner of doubt. Mr. H. was too late this season for the caravan that goes from Fezzan to Soudan, comprehending under that name Hourfes, Cashnou, Burnou, the great kingdom near the Niger. Meanwhile he has sent from Tripoli, by another conveyance, not yet arrived, the journal of his present travels; and there is every reason to hope that he will accomplish his great undertaking of visiting the unknown central regions of Africa, especially from the following occurrences mentioned in his letter:—He was followed from Sewah by a large party sent to seize him, on suspicion of his being a French spy. But his manners and behaviour were so completely Moslem, and he approved himself so thoroughly master of the Koran, that he was released with blessings and alms as a good Mussulman, and sent forward on his journey,

#### CEMENTING OF BROKEN GLASS,

C. Pajot, of Charmes, lately transmitted to the Philomatic Society and the National Institute, small bits of glass of different qualities, which he had joined and soldered so firmly that the glass would rather break close to the joining than in the actual place. The form of the fracture does not at all prevent the operation. The line of junction is scarcely discernible, and in some places not visible. C. Pajot has not made his process known.

C. Swediaur informed the Society, that a person named Hollenweger, about twelve or fourteen years ago, had in his presence, and that of Lavoisier and Meunier, performed some experiments, by means of which he joined in a solid manner, so as to make the junction hardly visible, fragments of blown glass.

C. Chaptal said also that some person had shewn him, about fifteen years ago, a glass bottle, the neck of which was so perfectly soldered to the glass stopper, that, when cut through, the circle of junction was scarcely perceptible. The  
bottle

bottle contained liquor of flints, and had lain on its side for a long time. "After seeing this fact, said Chaptal, I conceived the possibility of soldering together two plates of glass. I explained my ideas on this subject, and shewed the bottle, in my public lectures; and I am of opinion, that, by gradually withdrawing the dissolving alkali, it might be possible to unite such plates."

Before we dismiss this article, we beg leave to remark, that there appears to be nothing so wonderful in joining broken pieces of glass, in the way above described, as to justify Pajot in concealing the process. A little reasoning will lead any one to it. All that is necessary is to interpose, between the parts, a glass ground up like a pigment, but of easier fusion than the pieces to be joined, and then exposing them to such a heat as will fuse the cementing ingredient, and make the pieces agglutinate without being themselves fused. A glass fit for the purpose of cementing broken pieces of flint glass, may be made by fusing some of the same kind of glass, previously reduced to a powder, along with a little red lead and borax, or with the borax only.

#### DIED.

At Edinburgh, on the 6th inst. Dr. Joseph Black, one of the Physicians to his Majesty for Scotland, Professor of Chemistry in the University of Edinburgh, and a Member of the different respectable Medical and Literary Societies in Europe. The noble science of Chemistry has received many valuable improvements and elucidations from the genius and industry of the learned Dr. Black, which will transmit his name with honour to the latest posterity.

His papers were found in such perfect order by his executors, that we understand they intend speedily to publish them, with a life of the author.

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#### *Errata in our Last.*

In the description of Mr. Howard's Furnace, last line, (p. 192,) for *Windsor* read *fire* bricks.

Page 198, line 26, for *typani* read *tympani*.

In the description of the monument to Count Rumford, p. 206, line 17, read

At the creative glance of Charles Theodore,  
Rumford, the friend of mankind, &c.

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THE  
PHILOSOPHICAL MAGAZINE.

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JANUARY 1800.

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- I. *A Communication from GEORGE PEARSON, M.D. F.R.S. &c. Physician to St. George's Hospital, &c. concerning the Eruptions resembling the Small-Pox, which sometimes appear in the Inoculated Vaccine Disease.*

FOR reasons, the explanation of which would lead into too long a detail, and which, indeed, cannot be given with perfect propriety in this sheet; I feel myself compelled to publish some observations concerning the eruptions which appear, in some instances, in the cow-pock by inoculation.

Although the new inoculation in the present year has been, I think, sufficiently extensive to manifest the advantages of it over that for the small-pox, so that it is not likely to be ever totally laid aside; yet the unexpected appearance of eruptions has inclined many persons to be of opinion that no beneficial consequences can be produced by this practice, or, at least, that such consequences at best seem to be problematical. It may be useful to observe, that some of the Advocates for the cow-pock inoculation contend, that eruptions are never produced by it; accordingly, they assert, that, in these eruptive cases, the disorder was not the cow-pock, but the small-pox; the variolous poison having either been inserted inadvertently, or the constitution having been af-

fectéd by it casually. To justify what is advanced, it is incumbent on the Assertors either to prove that such errors have been committed, or at least they ought to be able to oppose equally extensive experience to that of the adverse party: for no sound reasoner will consider opinions, which are only founded in conjecture, to be demonstrated truths. I will not, however, take upon myself the unrequired task of attempting to vindicate others from the above charges; but I shall only perform a duty in stating the result of my own experience with regard to the point in question; conceiving that by this means the truth may be brought to light, if aided by the evidence from experience of future inquirers.

In the course of my practice the latter end of February, and in March following, I distinctly recollect four cases in which I first saw eruptions from the vaccine inoculation resembling so much those of the small-pox, that I should not have hesitated to consider them as belonging to this disease, if I had not excited them by a different poison from the variolous. I observed, however, at that time, some appearances of these eruptions different from those which usually occur in the small-pox. Almost all the eruptions, in the stage of desiccation, afforded shining, smooth, black or reddish-brown scabs; very few of them having previously suppurated. Finding, in two other instances, that the matter from the inoculated pustule of these patients produced a similar eruptive disorder, and also the same being the event in the practice of two or three of my correspondents, whom I had furnished with matter from the above eruptive cases, I from that time used matter only from the cases in which no eruptions appeared. After this precaution, no eruptive cases resembling the small-pox occurred in my practice during the whole of last summer and the present winter. I say, no cases occurred resembling the small-pox; but certainly eruptions, in number from a single one to about a dozen, which were large, red, hard pimples, with little or no lymph, and never with any pus, occurred, probably, in one case out of twenty or thirty. These spots, so unlike the small-pox, produced no trouble; and were of such a short duration, that, when I speak of eruptions, I do not include them in the number; I include in the account those only in which the

eruptions

eruptions resembled the small-pox: nor do I reckon among the eruptive cases those in which, now and then, a rash broke out about the 14th day after inoculation; and which was as troublesome as the *Urticaria*. My experience, then, with respect to the cases of eruptions being diminished in number by avoiding inoculation with matter of similar eruptive cases, coincides with Dr. Woodville's; and confirms what he has already so usefully communicated to the public. It was obvious to suspect, on the first occurrence of the eruptive cases, that variolous matter, in an unobserved way, but from sources which could not even be conjectured, had been introduced into the constitution instead of the vaccine poison. This conjecture, in spite of the clearest evidence of sense respecting the nature of the matter used, received some countenance from the non-appearance of eruptions as above stated; but, from the occurrence of such cases, in the practice of other Inoculators, in the last autumn and this winter, I think it is very unreasonable to doubt any longer, that, either on account of peculiar states of the human animal œconomy, or on account of some co-operating agents, the genuine vaccine poison does now and then produce a certain variety of the cow-pock, characterised by the appearance of pustules, like those of the variola. I have good evidence also to shew, that even in the hands of those very Inoculators, who a little time ago would not allow that the vaccine poison could produce eruptions, such cases have lately occurred.

In the month of October last I inoculated a child two years of age with the vaccine poison. The original matter, which had produced this matter, I took from the cow in March last; since which time the vaccine disease had been excited by it, in my hands, in a great number of patients. The vaccine disease took place with the usual appearances, in the inoculated part, and affected the whole constitution in the ordinary manner; but a few eruptions broke out on the second or third day after the slight fever; they were, however, only the red large pimples above mentioned, not at all like the small-pox. Mr. Keate carried matter from this child to Brighthelmstone, where Mr. Barret inoculated two children, who took the disease; and from one of these Mr. Keate ino-

culated *three* children. They all had the usual fever about the eighth day, and all had a number of eruptions, except one, which had only five or six, and these dried on the fifth day. This last case was probably that which Mr. Keate informs me had, in the inoculated part, the genuine vaccine pustule; but in all the others Mr. Barret observed that in the inoculated part the pustule was ragged at the edges, and flat, more resembling the variolous pustule. Matter from these patients was sent to Petworth, where Mr. Andre informs me he inoculated with it *fourteen* children. They all took the disease, and had eruptions like the variolous. *Three* children at the breast had from *three* to *twelve* pustules. The remaining *eleven* children had from fifty to several hundred eruptions. The state of the arms, and the characters of the pustules of the inoculated part, are not mentioned. None of the above patients died, nor is any mention made by Mr. Keate, Mr. Barret, and Mr. Andre, even of any apprehension of danger. Dr. Thornton sent me lately a case with eruptions, produced by matter which I originally took from a cow. According, then, to experience, we draw these conclusions:—

1. That in certain constitutions, or under the circumstances of certain co-operating agents, the vaccine poison produces a disease resembling the small-pox; and of course the pustule in the inoculated part is very different from that of the vaccine pock ordinarily occurring, and the eruptions resemble very much, if not exactly, some varieties of the small-pox.
2. That in some instances these eruptions have occurred although the inoculated part exhibited the genuine vaccine pustule.
3. That the matter of such eruptive cases, whether taken from the inoculated part, or from other parts, produces universally, or at least generally, similar eruptive cases, and has not, I believe, been seen to go back, by passing through different constitutions, to the state in which it produces what is called the genuine vaccine disease.
4. That eruptions, of a different appearance from variolous ones, sometimes occur in the true cow-pock,

Now, whether the vaccine poison, when it produces these cases resembling the small-pox, has really become, by composition or decomposition, variolous matter, is undetermined.

If

If this should be found to be the case by future experiments, still we must consider the two poisons as of distinctly different species, on account of the different characters of the pustule in the small-pox and cow-pox; although, as just said, by the combination of some other substance with the cow-pox poison, or by the separation of some one of the constituent ingredients of this poison, the variolous poison may be produced. To illustrate this theory let it be considered, that magnesia and sulphate of magnesia are different species of substances; although they agree in some of their principal effects on the human constitution, and in other properties; but, by the union of magnesia with sulphuric acid, it becomes sulphate of magnesia. Or the illustration may be given conversely. As, then, we have distinct denominations for these two substances, so we ought to have them for the two poisons, and the two different diseased states they produce, namely, the cow-pox and the small-pox. Accordingly, Dr. Odier, of Geneva, whose powers as a Dialectician, and whose acuteness as a Philologer, I can attest from the period of his academical studies, has "baptised" (to use the language of Dr. De Carro,) the new disease *La Vaccine*, or *Vaccina*, rejecting the absurd name of the English, *variolæ vaccinæ*.

But, to return to the immediate question under discussion, granting that eruptions are liable to be produced by the inoculation of the cow-pox, what difference ought this accident to make in our estimate of the value of the new practice, from the estimate on the supposition that no such eruptions would occur? I apprehend the value is hereby depreciated, but not to such a degree as to create any reasonable apprehensions of the failure of the Vaccine Inoculation in superseding and extinguishing the small-pox. Because,

1. If the precaution be taken of avoiding the inoculation with matter from eruptive cases, as far as I have seen, not above one case, with variolous-like eruptions, will be produced among 200 instances of inoculated cow-pox.
2. These eruptive cases are, as far as I have observed, like the ordinary kinds of inoculated small-pox.
3. I have seen no disfigurations of the skin from this variety of cow-pox; but I think it just to acknowledge that,  
from

from the experience I have had, no practitioner can answer for such cases not occurring in any instance, and as danger is always in proportion to the number of eruptions, which number no one can pretend to limit, it is evident that the chance of life during this disease is lessened, although but a very little. Provided this statement be made to the patient, it does not appear to me that the fact of the liability to eruptions ought to impede the progress of the Vaccine Inoculation; but if, on the contrary, the patient is assured that such eruptions will not occur, there is good ground for the practice falling into discredit, or at least for many persons, with reason, being discontented.

After this representation of an unfavourable part of the history of the cow-pock, it is consolatory to be able to counterpoise it with some new facts, which, like that of the eruptions, have been discovered in the course of further experience. It now appears that a person who has had the small-pox, is not susceptible of the cow-pox by inoculation\*; nor is a person susceptible of the constitutional affection from the cow-pox poison more than once. On the whole, then, we have gained perhaps as much as we have lost since the publication of the original account: and unless some new adverse facts shall be discovered, and considering that the public will adopt a practice which is so manifestly to their interest, the change effected in the practice of medicine will be so eminently memorable, *that the introduction of the Vaccine Inoculation must become an epoch in the history of physic.*

II. *Observations respecting the Earthquake which took place in Peru in the year 1797.* By M. CAVANILLES\*.

THERE are reckoned to be more than sixteen volcanoes in the kingdom of Quito which are in a continual state of eruption, and which throw up thick vapours, often mixed

\* The cases of milkers with chapt hands being repeatedly affected by the cow-pox poison in the *casual way*, whether they had undergone the small-pox or not, probably occasioned the error of the conclusions here alluded to.

† From the *Journal de Physique*, Fructidor an. 7.



with flames, either from their craters or through their lateral fissures. In the midst of the most profound calm there is frequently heard a dreadful bellowing noise, the forerunner of earthquakes, to which this part of the world is often exposed. After the year 1791, this noise was frequently heard in the neighbourhood of the mountain of Tunguragua. Antonio Pineda and Née, the two naturalists employed in the expedition round the world, when examining the declivity of this volcano, the lava of which had been hardened more by the internal fire than by the ardour of the sun, were struck with terror by the horrible sound which they heard, and the heat which they experienced. Pineda, that valuable member of society, whose premature death is still deplored by the friends of science, foretold that a terrible eruption was preparing in the mountain of Tunguragua; and his conjectures were confirmed by the event. On the 4th of February 1797, at three quarters past seven in the morning, the summit of the volcano was more free from vapours than usual; the interior part of the mountain was agitated by frequent shocks, and the adjacent chains burst in such a manner, that in the space of four minutes an immense tract of country was convulsed by an undulating movement. Never did history relate the effects of an earthquake so extraordinary, and never did any phenomenon of Nature produce more misfortunes, or destroy a greater number of human beings. A number of towns and villages were destroyed in a moment: some of them, such as Riobamba, Quero, Pelileo, Patate, Pillaro, were buried under the ruins of the neighbouring mountains; and others in the jurisdictions of Harnbata, Latacunga, Guaranda, Riobamba, and Alausi, were entirely overthrown. Some sustained prodigious loss by the gulphs which were formed, and by the reflux of rivers intercepted in their course by mounds of earth; and others, though in part saved, were in such a shattered state as to threaten their total ruin. The number of persons who perished during the first and succeeding shocks are estimated at 16,000. At ten o'clock in the morning, and four in the afternoon, the same day, (February 4,) after a dreadful noise, the earth was again agitated with great violence, and it did not cease to shake, though

though faintly, for the whole month of February and March; but, at three quarters past two in the morning of the 5th of April, the villages already ruined were again exposed to such violent shocks as would have been sufficient to destroy them. This extraordinary phenomenon was felt throughout the extent of 140 leagues from east to west, from the sea as far as the river Napo; and without doubt farther, for we are little acquainted with these districts which are inhabited by the savages. The distance north-east and south-west between Popajan and Piura, is reckoned to be 170 leagues; but in the centre of that district, 1 degree 16.6 from these places, is situated the part totally destroyed, and which comprehends forty leagues from north to south between Guarandam and Machache, and twenty leagues from east to west. But, as if an earthquake alone had not been sufficient to ruin this fertile and populous country, another misfortune, hitherto unknown, was added. The earth opened, and formed immense gulphs; the summits of the mountains tumbled down into the valleys, and from the fissures in their sides there issued an immense quantity of fetid water, which in a little time filled up valleys a thousand feet in depth and six hundred in breadth. It covered the villages, buildings, and inhabitants; choaked up the sources of the purest springs, and, being condensed by desiccation, in the course of a few days, into an earthy and hard paste, it intercepted the course of rivers, made them flow backwards for the space of eighty-seven days, and converted whole districts of dry land into lakes. Very extraordinary phenomena, which will doubtless be one day mentioned in history, occurred during these earthquakes; I shall, however, content myself with mentioning only two of them. At the same moment that the earth shook, the lake of Quirotoa, near the village of Infiloc, in the jurisdiction of Lacatunga, took fire, and the vapour which rose from it suffocated the cattle and flocks that were feeding in the neighbourhood. Near the village of Pelileo, a large mountain named Moya, which was overturned in an instant, threw out a prodigious stream of the before-mentioned thick fetid matter, which destroyed and covered the miserable remains of that city. Naturalists will

one

one day find, in these ravaged countries, objects worthy of their researches. Fragments of the minerals and earths of Tunguragua are about to be transported to Spain: but it is not in such fragments that we ought to search for the cause of these surprising phenomena; we must visit the country itself, where this conflict of the elements took place, and where the ruins it occasioned are still to be seen\*.

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III. *Experiments to determine the Quantity of Tanning Principle and Gallic Acid contained in the Bark of various Trees.* By GEORGE BIGGIN, Esq.†

THE bark of trees contains the astringent principle called gallic acid, and also that principle which has a peculiar affinity to the matter of skin, and which, from the use to which it is applied, is called the tanning principle. But, in the present mode of tanning, bark is applied in *mass* to the skins; consequently, *both* principles are applied. It remains for examination, whether both principles are useful in the process of tanning; for, if they are not both useful, probably *one* is detrimental.

To a nobleman, whose zeal on every occasion by which the sciences or arts may receive illustration or improvement, is eminently conspicuous, and to whose public energy, as well as private friendship, I feel myself much indebted, to his Grace the Duke of Bedford, I owe the means of prosecuting some experiments on this subject. His Grace, by collecting a variety of barks, at Woburn, gave me an opportunity of making some experiments to ascertain the quantity of tanning principle and gallic acid each bark contained. For that purpose I made use of the following methods, according to the principles laid down by M. Seguin:—

By dissolving an ounce of common glue in two pounds of boiling water, I procured a mucilaginous liquor, which, as it contains the matter of skin in solution, is a test for the

\* The volcano of Tunguragua occasioned an earthquake in 1557.

† From the *Philosophical Transactions of the Royal Society of London.*

tanning principle. By a saturated solution of sulphat of iron, I obtained a test for the gallic acid.

I then took one pound of the bark I meant to try, ground as for the use of tanners, and divided it into five parts, each part being put into an earthen vessel. To one part of this bark I added two pounds of water, and infused them for one hour. Thus I procured an infusion of bark, which I poured on the second part of the bark, and this strengthened infusion again on the third part, and so on to the fifth. But, as a certain portion of the infusion will remain attached to the *wood* of the bark after the infusion is poured or drawn off, I added a third pound of water to the first part, and then followed up the infusion on the several parts till the three pounds of water, or so much of them as could be separated from the bark, were united in the fifth vessel; from which I generally obtained about one pint of strong infusion of bark \*.

To a certain quantity of this infusion, I added a given measure of the solution of glue, which formed an immediate precipitate, that may be separated from the infusion by filtering paper. When dried, it is a substance formed by the chemical union of the matter of skin with the tanning principle, and is, in fact, a powder of leather. By saturating the infusion with the solution of glue, the whole of the tanning principle may be separated by precipitation.

#### *For the Gallic Acid.*

To the pound of bark left in the earthen vessels, and already deprived of its tanning principle by these *quick* infusions, I added a *given* quantity of water, to procure a strong infusion of the gallic acid, which requires a longer time, (say forty-eight hours.) This infusion, when obtained pure†, affords little signs of the presence of the tanning principle,

\* The specific gravity of this infusion was ascertained by an hydrometer whose gradations are inverse to those of a spirit hydrometer.

† It is hardly possible, from the intimate connection of the two principles, to separate them entirely by infusion: in the infusion of tanning principle, there will always exist a little gallic acid; and, in an infusion of gallic acid, a little tanning principle will commonly be present, unless the infusion of gallic acid is very weak, and procured by a third or fourth watering.

when

when tried by the test of the solution of glue; but, with the solution of sulphat of iron, it gives a strong black colour, (the common black dye,) which differs in density according to the quality of the bark: this may be further proved, by boiling a skain of worsted in the dye, by which the gradations of colour will be very perceptibly demonstrated.

Having thus obtained a point of comparison; by making a similar infusion, under similar circumstances, of any bark, or vegetable substance, and paying strict attention to the specific gravity of the infusion, the quantity of precipitate of leather, and the density of colour produced by given quantities of one or the other test, the result will be, a comparative statement of the respective powers of any bark, or vegetable substance. This comparative statement I conceive to be sufficient for all commercial purposes.

As oak bark is the usual substance employed in the trade of tanning, if a quantity of tanning principle is found to be contained in any other bark or vegetable, the commercial utility of that bark or vegetable may be determined, by comparing its quantity of tanning principle and price with those of oak bark.

For an accurate chemical analysis, I have tried a variety of acids, and simple and compound affinities; and, having pursued the above experiments at the same time that I was employed on some in dyeing, I found the muriat of tin (the method of using which is described by Mr. Proust in the *Annales de Chimie*,) very convenient. A solution of it being added to the infusion of bark, forms a precipitate with the tanning principle, leaving the gallic acid suspended: the precipitate is of a fawn colour, and is composed of tanning principle and oxydated tin.

By these means I have been enabled to form a comparative scale of barks; which, however, I do not produce as accurate. Oak bark, in its present state, as procured for commercial purposes, differs very much in quality, from accidental circumstances: the season of the year in which it is collected occasions a still more important difference, consequently the scale now produced must be very imperfect; but I am of opinion, that, by the pursuits of scientific men,

who may be inclined to investigate this subject more fully, a very accurate scale may hereafter be formed.

In the following scale, I have taken Sumach as the most powerful in the comparative statement; leaving, however, a few degrees for a *supposed maximum of tanning principle*, which I reckon twenty.

### SCALE OF BARKS.

Bark of	Gallic acid, by colour.	Tanning prin- ciple, by hy- drometer.	Tanning principle, (in grains,) from half a pint of infu- sion and an ounce of solution of glue.
Elm * - - -	7	2,1	28
Oak, cut in winter	8	2,1	30
Horse chestnut -	6	2,2	30
Beech - - -	7	2,4	31
Willow (boughs)	8	2,4	31
Elder - - -	4	3,0	41
Plum-tree - - -	8	4,0	58
Willow (trunk) -	9	4,0	52
Sycamore - - -	6	4,1	53
Birch - - -	4	4,1	54
Cherry-tree - -	8	4,2	59
Sallow - - -	8	4,6	59
Mountain ash -	8	4,7	60
Poplar - - -	8	6,0	76
Hazel - - -	9	6,3	79
Ash - - -	10	6,6	82
Spanish chestnut -	10	9,0	98
Smooth oak - -	10	9,2	104
Oak, cut in spring	10	9,6	108
Huntingdon or Lei- cester willow -	10	10,1	109
Sumach - - -	14	16,2	158

It is to be observed, that the barks do not keep any respective proportion in the quantity of gallic acid and tanning principle contained in each; which is an evidence of the

\* The infusion of elm was so loaded with mucilage that it was with difficulty I could separate the tanning principle, or try the specific gravity.  
distinctness

distinctness of principle, and may perhaps open a new field for saving oak bark in dyeing, as the willows, fallow, ash, and others, produce a very fine black. It is also worthy of observation, that the quantities of gallic acid and tanning principle do not differ in *equal* proportions between the winter and spring felled oaks. This fact may lead to the discrimination of the proper time for cutting; which is, probably, when the sap has completely filled and dilated that part of the vegetable intended for use. This will make a difference in the season of cutting oak, elm, and other trees, shrubs, &c. Leaves should be taken when arrived at their full size, and then dried under cover; for, as the tanning principle is so soluble, and the substance that contains it so thin, (in a leaf,) the dew alone might dissolve it.

Finally, as the gallic acid does not seem to combine with the matter of skin, and as its astringency will corrugate the surface, we may, I think, conclude, that its presence in tanning is not only useless, but detrimental.

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IV. *Extract of a Memoir on the Grecian Method of dyeing Cotton Yarn Red.* By C. FELIX\*.

THAT beautiful red dye given to cotton in the Ottoman empire, is known in Europe under the name of Turkey red, Levant red, or Adrianople red. As it is believed among us that this colour results chiefly from the processes employed in the dyeing, I shall give an account of those followed in the Grecian manufactories. It must, however, be observed, that in these manufactories the workmen dye at one time a mass of skins weighing thirty-five occas; each occa being equal to about fifty ounces.

The first process is that of cleaning the cotton, for which purpose three leys are employed; one of soda, another of ashes, and a third of lime. The cotton is thrown into a tub, and moistened with the liquor of the three leys in equal quantities; it is then boiled in pure water, and washed in running water.

\* From the *Annales de Chimie*, No. 92.

The second bath given to the cotton is composed of soda and sheep's dung dissolved in water. To facilitate the solution the soda and dung are pounded in a mortar. The proportions of these ingredients employed, are, one oca of dung, six of soda, and forty of water. When the ingredients are well mixed, the liquor expressed from them is strained, and being poured into a tub, six occas of olive oil are added to it, and the whole is well stirred till it becomes of a whitish colour, like milk. The cotton is then besprinkled with this water, and when the skains are thoroughly moistened, they are wrung, pressed, and exposed to dry. The same bath must be repeated three or four times, because it is this liquor which renders the cotton more or less fit for receiving the dye. Each bath is given with the same liquor, and ought to continue five or six hours. It is to be observed that the cotton, after each bath, must be dried without being washed, as it ought not to be rinsed till after the last bath. The cotton is then as white as if it had been bleached in the fields.

The bath of sheep's dung is not used in our manufactories; it is a practice peculiar to the Levant. It may be believed that the dung is of no utility for fixing the colours; but it is known that this substance contains a great quantity of volatile alkali, in a disengaged state, which has the property of giving a rosy hue to the red. It is therefore probable that it is to this ingredient that the red dyes of the Levant are indebted for their splendour and vivacity. This much, at any rate, is certain, that the Morocco leather of the Levant is prepared with dog's dung; because it has been found that this dung is proper for heightening the colour of the lack. The bath of dung is followed by the process of galling.

The galling is performed by immersing the cotton in a bath of warm water, in which five occas of pulverised gall-nuts have been boiled. This operation renders the cotton more fit for being saturated with the colour, and gives to the dye more body and strength. After the galling comes aluming, which is performed twice, with an interval of two days, and which consists in dipping the cotton into a bath of water in which five occas of alum have been infused, mixed with five occas of water alcalised by a ley of soda. The aluming  
must



must be performed with care, as it is this operation which makes the colouring particles combine best with the cotton, and which secures them in part from the destructive action of the air. When the second aluming is finished, the cotton is wrung; it is then pressed, and put to soak in running water, after being inclosed in a bag of thin cloth.

The workmen then proceed to the dyeing.—To compose the colours they put in a kettle five occas of water and thirty-five occas of a root which the Greeks call *ali-zari*, or painting colour, and which in Europe is known under the name of *madder*. The madder, after being pulverised, is moistened with one occa of ox or sheep's blood. The blood strengthens the colour, and the dose is increased or lessened according to the shade of colour required. An equal heat is maintained below the kettle, but not too violent; and when the liquor ferments, and begins to grow warm, the skains are then gradually immerfed before the liquor becomes too hot. They are then tied with packthead to small rods, placed crosswise above the kettle for that purpose, and when the liquor boils well, and in an uniform manner, the rods from which the skains were suspended are removed, and the cotton is suffered to fall into the kettle, where it must remain till two-thirds of the water is evaporated. When one-third only of the liquor remains, the cotton is taken out and washed in pure water.

The dye is afterwards brought to perfection by means of a bath alcalised with soda. This manipulation is the most difficult and the most delicate of the whole, because it is that which gives the colour its tone. The cotton is thrown into this new bath, and made to boil over a steady fire till the colour assumes the required tint. The whole art consists in catching the proper degree: a careful workman, therefore, must watch with the utmost attention for the moment when it is necessary to take out the cotton, and he will rather burn his hand than miss that opportunity.

It appears that this bath, which the Greeks think of so much importance, might be supplied by a ley of soap; and it is probable that saponaceous water would give the colour more brightness and purity.

When

When the colour is too weak, the Levantines know how to strengthen it by increasing the dose of the colouring substances; and when they wish to give it brightness and splendour, they employ different roots of the country, and, in particular, one named *saffari*, specimens of which I have sent to France.

The ali-zari, which is the principal colouring matter employed in the Greek dye-houses, is collected in Natolia, and is brought to Greece from Smyrna: some of it comes also from Cyprus and Mesopotamia. The superiority of this Levantine plant to the European madder is acknowledged by all those acquainted with the art of dyeing, and may arise from two causes; the manner in which it is cultivated, and the method employed for its desiccation.

As the ali-zari appears to be of a weaker constitution than the common madder: as its branches are more delicate, its leaves smoother and tenderer, and its stem more fragile, it is supported by sticks, as pease are among us. A well nourished stem acquires in this manner more consistence, and sends forth more roots. It is to be observed, that it is the woody part of the roots which affords the greatest quantity of colouring particles. The ali-zari is not collected till the fifth or sixth year, that is to say, when it has acquired its full strength; while in France and Zealand, where land is of more value than in the Levant, the inhabitants wish to reap too soon, and collect the madder before it comes to complete maturity.

The method employed in the desiccation contributes also to improve the quality of the ali-zari. The Levantines dry it in the open air; and this operation is easy in a country where great dryness prevails in the atmosphere, while, in our damp climates, we are obliged to dry the madder by stoves. Hence it happens that the smoke, which mixes itself with the cold air, and penetrates the roots, impregnates them with fuliginous particles, which alter the colouring substance; an accident which does not take place when the madder is dried without the assistance of fire. It is, however, possible, that the ali-zari and madder may never produce the same tone of colour, notwithstanding all the care that may be employed

in the culture and drying of the latter; because there may be as much difference between these two plants, so like to each other, as between the goats of France and those of Angora.

It is probable, also, that the superiority of the ali-zari over madder may arise only from its stem being tenderer, and by its consequently having a greater disposition to transform itself into succulent roots. In that case, several of our indigenous plants of the family of the *rubice* might be substituted in its place with advantage; such as the *gallium luteum* and *gallium flore albo*, found in abundance on the hills of Poitou, and the madder, which grows on both sides of the Alps, and which has been distinguished by the name of *rubia lævis Tourinensium*. All these plants give a red as beautiful and as pure as the best madder; but I do not know whether the quantity of their colouring matter is equal to their beautiful quality.

The chief manufactories for dyeing spun cotton red, established in Greece, are in Thessaly. There are some at Baba, Rapsani, Tournavos, Larissa, Pharsalia, and in all the villages situated on the sides of Ossa and Pelion. These two mountains may be considered as the alembics that distil the eternal vapours with which Olympus is crowned, and which distribute them throughout the beautiful valleys situated around them. Of these valleys, that of Tempe has at all times been distinguished by the beauty of its shady groves and of its streams. These streams, on account of their limpidness, are very proper for dyeing, and supply water to a great number of manufactories, the most celebrated of which are those of Ambelakia.

Ambelakia, on account of the activity which prevails in it, has a greater resemblance to a town of Holland than a village of Turkey. This village, by its industry, communicates life and activity to all the neighbouring country, and gives birth to an immense trade, which connects Germany with Greece in a thousand ways. Its population, which has been tripled within these fifteen years, amounts at present to 4000, and all these people exist by dyeing. None of those vices or cares produced by idleness are known here. The hearts of

the inhabitants are pure, and their countenances unclouded. Servitude, which degrades the countries watered by the Peneus, has not yet ascended to these hills: no Turk can reside or live among these people; and they govern themselves, like their ancestors, by their *protoyeros* and their own magistrates. Twice have the savage mussulmans of Larissa, envious of their ease and happiness, attempted to scale their mountains in order to plunder their houses; and twice have they been repulsed by hands which suddenly quitted the shuttle to assume the musket.

All hands, and even those of the children, are employed in the dye-houses of Ambelakia; and while the men dye the cotton, the women are spinning and preparing it. The use of wheels is not known in this part of Greece; all the cotton is spun on a distaff: the thread, indeed, is certainly not so round or equal, but it is softer, more silky, and more tenacious; it is less apt to break, and lasts longer; it is also more easily whitened, and more proper for being dyed. It is a pleasing spectacle to see the women of Ambelakia, each spinning from a distaff, and sitting conversing together on the threshold of their doors; but as soon as a stranger appears, they instantly retire and conceal themselves in their houses, manifesting, like Galatea, in their precipitate retreat, a desire of flying and of shewing themselves:—

*Et fugit ad salices, et se cupit ante videri.*

The eye can only have a cursory view of the shape of some of these women; but it still observes, with surprise, those slender and graceful forms of the ancient Grecian females, which served as models to the most beautiful statues in the world.

V. *An Account of the Wild Horses in Spanish America.*  
By D. FELIX AZARA\*.

THESE animals were originally carried from Spain by the first conquerors, and are of the Andalusian breed. They

\* From a work, not yet published, on the natural history of Paraguay, by Don Felix Azara, brother of the late Spanish ambassador at Paris. This extract is translated from the *Décade Philosophique*, No. 9, year 8.

chiefly frequent the southern part of the river de la Plata, as far as Rio Negro, the country of the Patagonians, &c. The wild horses of all these countries live in numerous herds, some of which, it is said, consist of ten thousand. As soon as they perceive domestic horses in the fields, they run towards them on a full gallop, pass through the middle of them, or near them, caress them, and invite them with a kind of grave and prolonged neighing. The domestic horses are soon seduced, unite themselves to the independent herd, and depart along with them. It happens not unfrequently that travellers are stopped on the road by the effect of this desertion. To prevent it, they halt as soon as they perceive these wanderers, watch their own horses, and endeavour to frighten away the others. In such cases the wild horses follow a certain kind of tactics; some are detached before, and the rest advance in a close column, which nothing can interrupt. If they are so alarmed as to be obliged to retire, they change their direction, but without suffering themselves to be dispersed: sometimes they make a great number of turns around those which they wish to seduce, in order to frighten them; at other times they retire after making one turn. These manœuvres are not employed during the night, for the wild horses then make no attempts. The author is ignorant whether any thing of the same kind takes place between one herd of wild horses and another for the purpose of recruiting their number. He asserts that Buffon is mistaken in saying that these wild horses have more strength and fleetness than the domestic horses of the country, and that they do not differ from the latter either in height or shape: but he indeed observes, that no comparison can be made between the independent and domestic state of these animals, as in that country both states are almost similar.

Those who possess *estancias*, or pastures for keeping domestic horses, place in them a certain number of mares, which are never broke or mounted. They remain during their whole lives in a state of perfect liberty; and for every thirty or forty mares there is a stallion, which enjoys the same independence. They are counted once or twice a week, in order that they may not stray from the habitation. But

too little care is employed in the choice of the stallions; and this, in all probability, is one of the principal causes why these horses, though left at full liberty, are neither so beautiful nor so good as those of Andalusia, from which they are descended. The industry of man improves the breed of those animals which he appropriates to his own use, and, by bringing their shape and organs to perfection, indemnifies them for that liberty of which he deprives them. Each stallion takes possession of a small troop of mares, which he keeps collected by pressing them with his chest, and by biting them if they do not obey with sufficient docility: the mares, on the other hand, remain attached to their sultans. If two stallions fight, the mares do not abandon the conquered for the conqueror, unless the former has shewn among them a deficiency of vigour.

The fillies remain with their mothers. When it is time to break the colts they are cut, for no person mounts a horse until that operation has been performed. After castration, a halter is put upon the animal intended to be broke; he is tied to a stake; a saddle is placed upon his back, and well girded, but without crupper or breast-leather, and a thong is tied round his under-lip, to which is fastened, on each side, a rein, in order to govern him. The horseman then mounts with large spurs, and rides out into the fields. At first the young horse capers and jumps until he is quite fatigued; after which he is brought back to the stake. This exercise is repeated several times in the course of the same day, and is renewed after the interval of several days until the courser capers no more. He is then employed as a broke horse, but with a halter only; a bridle is not put upon him till a year after, at which time he quits the name of *rodomont* to assume that of horse.

As soon as the horses are cut, they are separated from the mares, and put among the animals of the horse species used for service, which receive no other shelter or food than what they find in the fields. They are accustomed to live in one canton, which they never quit. Each unites himself to a companion, and with such intimacy that instances have been known of some of them, after running away, having  
returned

returned more than sixty leagues to rejoin their old friend. These friends know each other by their neighing, their smell, and the noise of their pace.

When the proprietors are desirous to prevent a numerous troop of domestic horses from separating, they place among them a young mare with a small bell, and which is then called the godmother. They all follow her, and they all know and seek for each other as members of the same society. The same effect may be produced by attaching the bell to one of the horses of the troop.

When the inhabitants have need of horses, a man on horseback, bearing a lance, proceeds towards a troop of these animals, and drives them into an inclosure formed of pallisades. A horseman then enters it, and, when he is within reach, entangles the horse which he wishes to catch with a kind of rope; for, though these horses are fit to be mounted, and said to be docile, they will not suffer themselves to be touched with the hand.

From what has been said, it may be readily seen that there is really very little difference between the habits of these wild horses and of those which live in a state of domesticity. It needs excite no wonder, therefore, that there should be very little in regard to their form, size, and qualities.

When the inhabitants wish to convert some of these wild horses into domestic ones, people mounted on horseback proceed towards a troop of the former; and when they approach them they throw some of the ropes already mentioned around their legs, so that, being prevented from running, they have time to secure them. They are then tied to a stake, or a tree, not by the four legs, as some have said, but with a simple halter made of leather. They are left two or three days without food or drink, are afterwards cut, and are then broke in the same manner as the domestic horses. The horse behaves then as if he had never been wild; but it is not true that they lose all desire of recovering their liberty. They readily unite with a troop of wild horses; and how can it be otherwise, since those even which have been reared in a state of domesticity have no repugnance to join them?

The proprietors of the *estancias*, or pastures destined for

those horses which have been tamed, endeavour not only to frighten away the wild horses, but even to exterminate them. With that view they beat the woods in quest of them, drive them, if possible, into ravines, and kill them by means of lances.

The Pampas eat their flesh, and particularly that of the fillies, colts, and mares; but they sometimes kill a very fat stallion to make a fire with his grease and bones, as in the country of Pampa wood is extremely scarce.

The Spanish author often contradicts Buffon, not only in regard to local observations, which is not astonishing, but also in regard to general ideas, such as the influence of climate, &c. D. Felix Azara refutes the assertion of the French naturalist, who ascribes more strength and fleetness to the wild than to the domestic horses of these countries. He even asserts, that he has not been correct in saying that each wandering troop submit, by common consent, to a chief, which serves as a guide; which regulates and directs their movements; forms them in the order of battle, by files, companies, squadrons, &c. The truth, according to the author, is, that each stallion appropriates to himself as many mares as he can, which he takes care of, and keeps collected; that he combats any other stallion which attempts to deprive him of them; and that each wandering troop consists, therefore, of a number of small distinct bodies, which sometimes unite into one.

In the great number of wild troops which the author saw, he never observed any other prevailing colours than bay, dark brown, and jet black. If it sometimes happens that a pied or dirty grey coloured individual is seen, or one of any other colour, it may with certainty be concluded that it is a domestic horse which has deserted. According to the author, there are ninety bay for one dark brown horse; and black horses are so uncommon, that one of them is scarcely seen in two thousand. He thence infers, that these three colours, bay, dark brown, and jet black, are a primitive mark, which distinguishes, at least in part, the horses which recover their liberty; that the first horse and mare which existed had one of these three colours, and most probably the bay, since it appears



appears that, among the wild horses, the black is becoming extinct, and that this will be the case also with the brown; that, taking the colour as an index, we might say, that the best breed of horses is the bay, then the brown, and next the black; all the other colours being inferior, as they are the result of more distant degradations from the primitive horse, which must have been the most perfect. Experience seems, in some measure, to confirm these conjectures; for, except in a few cases, which are of little consequence, the bays are the most esteemed, and the browns hold the next rank. He observes, that in France a prejudice is entertained against the last-mentioned colour, which he thinks unreasonable, and which in his opinion seems to shew that the French, in this respect, have not so much discernment as the Spaniards. These observations, and the inferences which the author draws from them, seem to weaken the confidence which might be placed in what has been said by Buffon, on the authority of Herodotus, Leo Africanus, and Marco Polo, of wild white horses said to have existed in Arabia and Numidia. We know how suspicious the testimony of the ancients is in regard to natural history, and that the authority of Buffon himself has little weight when he gives testimonies instead of observed facts.

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VI. *An Account of the Pearl Fishery in the Gulph of Manar, in March and April 1797.* By HENRY J. LE BECK, Esq.\*

FROM the accounts of the former pearl fisheries at Ceylon, it will be found, that none have ever been so productive as this year's. It was generally supposed that the renter would be infallibly ruined, as the sum he paid for the present fishery was thought exorbitant when compared with what had been formerly given: but this conjecture in the event appeared ill-founded, as it proved extremely profitable and lucrative.

The farmer this time was a Tamul merchant, who, for

\* From the *Asiatic Researches*, Vol. V.

the privilege of fishing with more than the usual number of donies or boats, paid between two and three hundred thousand Porto-novo pagodas; a sum nearly double the usual rent.

These boats he farmed out again to individuals in the best manner he could, but for want of a sufficient number of divers some of them could not be employed.

The fishing, which commonly began about the middle of February, if wind and weather allowed, was this year, for various reasons, delayed till the end of the month; yet, so favourable was the weather, that the renter was able to take advantage of the permission granted by the agreement, to fish a little longer than the usual period of thirty days.

The fishery cannot well be continued after the setting-in of the southern monsoon, which usually happens about the 15th of April, as, after that time, the boats would not be able to reach the pearl banks, and the water being then so troubled by heavy seas, diving would be impracticable; in addition to which, the sea-weed, a species of *fucus*, driven in by the southerly wind, and which spreads to a considerable distance from the shore, would be an impediment.

Many of the divers, being Roman Catholics, leave the fishery on Sundays to attend divine service in their church at Aripoo; but if either a Mahomedan or Hindoo festival happens during the fishing days, or if it is interrupted by stormy weather, or any other accident, this lost time is made up by obliging the Catholics to work on Sundays.

The fear of sharks, as we shall see hereafter, is also another cause of interruption. These, amongst some others, are the reasons that, out of two months, (from February till April,) seldom more than thirty days can be employed in the fishery.

As this time would be insufficient to fish all the banks, (each of which has its appropriate name, both in Dutch and Tamul,) it is carried on for three or four successive years, and a new contract annually made, till the whole banks have been fished; after which they are left to recover.

The length of time required for this purpose, or from one general fishing to another, has not yet been exactly determined; it was therefore a practice to depute some persons to  
visit

visit the banks annually, and to give their opinion, whether a fishery might be undertaken with any degree of success\*?

From various accounts, which I have collected from good authority, and the experience of those who assisted at such examinations, I conjecture, that every seven years such a general fishery could be attempted with advantage, as this interval seems sufficient for the pearl shells to attain their growth: I am also confirmed in this opinion by a report, made by a Dutch governor at Jafnas, of all the fisheries that have been undertaken at Ceylon since 1722; a translation of which is to be found in Wolfe's Travels into Ceylon. But the ruinous condition in which the divers leave the pearl banks at each fishery, by attending only to the profit of individuals, and not to that of the public, is one great cause that it requires twice the above-mentioned space of time, and sometimes longer, for rendering the fishing productive. They do not pay the least attention to spare the young and immature shells that contain no pearl; heaps of them are seen thrown out of the boats as useless on the beach between Manâr† and Aripoo: if these had been suffered to remain in their native beds, they would, no doubt, have produced many fine pearls. It might therefore be advisable to oblige the boat-people to throw them into the sea again before the boats leave the bank. If this circumspection, in sparing the small pearl shells to perpetuate the breed, was always observed, succeeding fisheries might be expected sooner, and with still greater success: but the neglect of this simple precaution will, I fear, be attended with similar fatal consequences here, as have already happened to the pearl banks on the coast of Persia, South America, and Sweden, where the fisheries are by no means so profitable at present as they were formerly.

Another cause of the destruction of numbers of both old and young pearl shells is the anchoring of so many boats

\* A gentleman, who assisted at one of the last visits, being an engineer, drew a chart of the banks, by which their situation and size are now better known than formerly.

† Manara, properly Manâr, is a Tamul word, and signifies a sandy river, from the shallowness of the sea at that place.

on the banks; almost all of them used differently formed, clumsy, heavy, wooden anchors, large stones, &c. &c. If this evil cannot be entirely prevented, it might, at least, be greatly lessened, by obliging them all to use anchors of a particular sort, and less destructive.

This season the Seewel Bank only was fished, which lies above twenty miles to the westward of Aripoo, opposite to the fresh water rivers of Moosalee, Modragam, and Pomparipoo. It has been observed, that the pearls on the north-west part of this bank, which consists of rock, are of a clearer water than those found on the south-east, nearest the shore, growing on corals and sand.

Condatchey is situated in a bay, forming nearly a half moon, and is a waste, sandy district, with some miserable huts built on it. The water is bad and brackish, and the soil produces only a few, widely scattered, stunted trees and bushes. Those persons who remain here during the fishery are obliged to get their water for drinking from Aripoo, a village with a small old fort, lying about four miles to the southward. Tigers, porcupines, wild hogs, pangolines, or the *Ceylon armadillos*, are, amongst other quadrupeds, here common. Of amphibia, there are tortoises, especially the *testudo geometrica*, and various kinds of snakes. A conchologist meets here with a large field for his enquiries. The presents which I made to the people employed in the fishery, to encourage them to collect all sorts of shells which the divers bring on shore, produced but little effect; as they were too much taken up in searching after the mother-of-pearl shells to pay attention to any other object. However, my endeavours were not entirely useless; I will specify here a few of the number I collected during my stay: different kinds of *pectines*\*, *palium porphyreum*, *solen radiatus*†, *Venus castrensis* Linn.‡ *astrea hyotis* §, *ostr. Forskolii*, *ostr. Malleus* ||, *mytilus hirundo* Linn. ¶, *spondilus crocius*, *pholas*

\* Scallops.

† Radiated razor-shell.

‡ Alpha cockle.

§ Double cock's-comb.

|| Hammer oyster; these were pretty large, but many broken, and some covered by a calcareous crust. It is very probable that among those there may be some precious white ones.

¶ Swallow muscle.

*puffinus*

*pusillus* Linn.\*, *mitra episcopalis* Linn., *lepas striata* Pennanti, (vide Zool. Brit.) *patella tricarinata* Linn., *bulla perfecta maculata*†, *barpa nobilis*, *porcellana salita* Rumph.‡, *strombus scorpio*, and other of inferior kinds. Amongst the *zoophytes*, many valuable species of *spongiæ*, *corallinæ*, *satulariæ*, &c. a great variety of sea-stars, and other marine productions, that cannot be preserved in spirits, but should be described on the spot. These, as well as the description of the different animals inhabiting the shells, are the more worthy of our attention, and deserve farther investigation, as we are yet very deficient in this branch of natural history.

During the fishing season, the desert, barren place Con-datchey offers to our view a scene equally novel and astonishing. A heterogeneous mixture of thousands of people, of different colours, countries, casts, and occupations; the number of tents and huts erected on the sea-shore, with their shops or bazars before each of them; and the many boats returning on shore in the afternoon, generally richly laden; all together form a spectacle entirely new to an European eye. Each owner runs to his respective boat as soon as it reaches the shore, in hopes of finding it fraught with immense treasure, which is often much greater in imagination than in the shell; and though he is disappointed one day, he relies with greater certainty on the next, looking forward to the fortune promised him by his stars, as he thinks it impossible for the astrological predictions of his Brâhmen to err.

To prevent riot and disorder, an officer with a party of Malays is stationed here. They occupy a large square, where they have a field-piece, and a flag-staff for signals.

Here and there you meet with brokers, jewellers, and merchants, of all descriptions; also futtlers, offering provisions and other articles to gratify the sensual appetite and luxury. But by far the greater number are occupied with the pearls. Some are busily employed in assorting them; for which purpose they make use of small brass plates perforated with holes of different sizes; others are weighing, and offering them to

\* The wood-piercer.

† Diving snail, (Grew, Mus.)

‡ Salt-coury, Kl.

the purchaser; while others are drilling or boring them, which they perform for a trifle.

The instrument these people carry about with them for this purpose is of a very simple construction, but requires much skill and exercise to use it; it is made in the following manner: The principal part consists of a piece of soft wood, of an obtuse, inverted, conical shape, about six inches high and four in diameter in its plain surface; this is supported by three wooden feet, each of which is more than a foot in length. Upon the upper flat part of this machine are holes or pits for the larger pearls, and the smaller ones are beat in with a wooden hammer. On the right side of this stool, half a cocoa-nut shell is fastened, which is filled with water. The drilling instruments are iron spindles, of various sizes, adapted to the different dimensions of the pearls, which are turned round in a wooden head by a bow. The pearls being placed on the flat surface of the inverted cone, as already mentioned, the operator, sitting on a mat, presses on the wooden head of his instrument with the left hand, while, with his right, he moves the bow which turns round the moveable part of the drill; at the same time he moistens the pearl, occasionally dipping the little finger of the same hand into the water of the cocoa-nut shell with a dexterity that can only be attained by constant practice.

Amongst the crowd are found vagabonds of every description, such as the Larams, Andee or Hindu monks, fakirs, beggars, and the like, who are impertinently troublesome. Two of these wretches particularly attracted the attention of the mob, though their superstitious penance must have disgusted a man of the least reflection: one had a gridiron, of one foot and a half long and the same in breadth, fastened round his neck, with which he always walked about, nor did he take it off either when eating or sleeping; the other had fastened round that member which decency forbids me to mention, a brass ring, and fixed to it was a chain, of a fathom in length, trailing on the ground; the links of this chain were as thick as a man's finger, and the whole was exhibited in a most scandalous manner.

The pestilential smell occasioned by the numbers of putrefying

lying pearl fishes, renders the atmosphere of Condatchey so insufferably offensive when the south-west wind blows, that it sensibly affects the olfactory nerves of any one unaccustomed to such cadaverous smells. This putrefaction generates immense numbers of worms, flies, muskitoes, and other vermin, all together forming a scene strongly displeasing to the senses.

Those who are not provided with a sufficient stock of money suffer great hardships, as not only all kinds of provisions are very dear, but even every drop of good water must be paid for. Those who drink the brackish water of this place are often attacked by sickness. It may easily be conceived what an effect the extreme heat of the day, the cold of the night, the heavy dews, and the putrid smell, must have on weak constitutions. It is therefore no wonder, that of those who fall sick many die, and many more return home with fevers, fluxes, or other equally fatal disorders.

The many disappointments usually experienced by the lower classes of men in particular, make them often repent of their coming here. They are often ruined, as they risk all they are worth to purchase pearl shells: however, there are many instances of their making a fortune beyond all expectation. A particular circumstance of this kind fell within my own observation: a day labourer bought three oysters \* for a copper fanam, (about the value of two-pence,) and was so fortunate as to find one of the largest pearls which the fishery produced this season.

The donies appointed for the fishery are not all procured at Ceylon; many came from the coasts of Coromandel and Malabar, each of which has its distinguishing number. About ten o'clock at night a gun is fired as a signal, when they sail from Condatchey with an easterly or land wind, under the direction of a pilot. If the wind continues fair, they reach the bank before day, and begin diving at sun-rise, which they continue till the west or sea-breeze sets in, with which they return. The moment they appear in sight, the

\* The East India pearl shell is well known to be the *matrix perlarum* (mother-of-pearl) of Rumphius, or the *Mytilus margaritiferus* of Linneus; consequently, the general term pearl-oyster must be erroneous: however, as it has long been in common use, I hope to be excused for continuing it.

colours are hoisted at the flag-staff, and in the afternoon they come to an anchor; so that the owners of the boats are thereby enabled to get their cargoes out before night, which may amount to 30,000 oysters, if the divers have been active and successful.

Each boat carries twenty-one men and five heavy diving-stones, for the use of ten divers, who are called in Tamul *kooly kârer*; the rest of the crew consists of a tandel, or head boatman, and ten rowers, who assist in lifting up the divers and their shells.

The diving-stone is a piece of coarse granite, a foot long, six inches thick, and of a pyramidical shape, rounded at the top and bottom. A large hair rope is put through a hole in the top. Some of the divers use another kind of stone, shaped like a half moon, to bind round their belly, so that their feet may be free. At present these are articles of trade at Con-datchey. The most common, or pyramidical stone, generally weighs about thirty pounds. If a boat has more than five of them, the crew are either corporally punished or fined.

The diving, both at Ceylon and at Tutucorin, is not attended with so many difficulties as authors imagine. The divers, consisting of different casts and religions, (though chiefly of Parrawer\* and Mussulmans,) neither make their bodies smooth with oil, nor do they stop their ears, mouths, or noses with any thing, to prevent the entrance of salt water. They are ignorant of the utility of diving-bells, bladders, and double flexible pipes. According to the injunctions of the shark conjurer they use no food while at work, nor till they return on shore and have bathed themselves in fresh water. These Indians, accustomed to dive from their earliest infancy, fearlessly descend to the bottom, in a depth of from five to ten fathoms, in search of treasures. By two cords a diving-stone and a net are connected with the boat. The diver, putting the toes of his right foot on the hair rope of the diving-stone, and those of his left on the net, seizes the two cords with one hand, and, shutting his nostrils with the other, plunges into the water. On reaching the bottom, he hangs the net round his neck, and collects into it the pearl shells

\* Fishermen of the Catholic religion.



as fast as possible during the time he finds himself able to remain under water, which usually is about two minutes. He then resumes his former posture, and making a signal, by pulling the cords, he is immediately lifted into the boat. On emerging from the sea, he discharges a quantity of water from his mouth and nose, and those who have not been long enured to diving frequently discharge some blood; but this does not prevent them from diving again in their turn. When the first five divers come up, and are respiring, the other five are going down with the same stones. Each brings up about one hundred oysters in his net, and, if not interrupted by any accident, may make fifty trips in a forenoon. They and the boat's crew get generally from the owner, instead of money, a fourth of the quantity which they bring on shore; but some are paid in cash, according to agreement.

The most skilful divers come from Collish, on the coast of Malabar; some of them are so much exercised in the art, as to be able to perform it without the assistance of the usual weight, and for a handsome reward will remain under water for the space of seven minutes: this I saw performed by a Caffry boy, belonging to a citizen at Karical, who had often frequented the fisheries of these banks. Though Dr. Halley deems this impossible, daily experience convinces us, that, by long practice, any man may bring himself to remain under water above a couple of minutes. How much the inhabitants of the South Sea Islands distinguish themselves in diving we learn from several accounts; and who will not be surpris'd at the wonderful Sicilian diver Nicholas, surnamed the Fish\*?

Every one of the divers, and even the most expert, entertain a great dread of the sharks, and will not, on any account, descend until the conjurer has performed his ceremonies. This prejudice is so deeply rooted in their minds, that the government was obliged to keep two such conjurers

\* According to Kircher, he fell a victim amongst the Polypes in the gulph of Charybdis, on his plunging, for the second time, in its dangerous whirlpool, both to satisfy the curiosity of his king, Frederic, and his inclination for wealth. I will not pretend to determine how far this account has been exaggerated.

always in their pay, to remove the fears of their divers. Thirteen of these men were now at the fishery from Ceylon and the coast, to profit by the superstitious folly of these deluded people. They are called in Tamul *Pillāl Kadtūr*, which signifies one who binds the sharks, and prevents them from doing mischief.

The manner of enchanting consists in a number of prayers learned by heart, that nobody, probably not even the conjurer himself, understands, which he, standing on the shore, continues muttering and grumbling from sun-rise until the boats return: during this period they are obliged to abstain from food and sleep, otherwise their prayers would have no avail; they are, however, allowed to drink, which privilege they indulge in a high degree, and are frequently so giddy as to be rendered very unfit for devotion. Some of the conjurers accompany the divers in their boats; which pleases them very much, as they have their protectors near at hand. Nevertheless, I was told, that in one of the preceding fisheries a diver lost his leg by a shark; and when the head conjurer was called to an account for the accident, he replied, that an old witch had just come from the coast, who, from envy and malice, had caused this disaster by a counter-conjuration, which made fruitless his skill, and of which he was informed too late: but he afterwards shewed his superiority by enchanting the poor sharks so effectually, that, though they appeared in the midst of the divers, they were unable to open their mouths. During my stay at Condatchey, no accident of this kind happened. If a shark is seen, the divers immediately make a signal, which on perceiving, all the boats return instantly. A diver who trod upon a hammer-oyster, and was somewhat wounded, thought he was bit by a shark, consequently made the usual signal; which caused many boats to return; for which mistake he was afterwards punished.

The owners of the boats \* sometimes sell their oysters, and at other times open them on their own account. - In the latter case, some put them on mats in a square, surrounded with a

\* These are the individuals which farm one or more boats from the renter; and though they are in possession of them only during the fishery, they are commonly called the owners of the boats.

fence; others dig holes of almost a foot deep, and throw them in till the animal dies; after which they open the shells, and take out the pearls with more ease. Even these squares and holes are sold by auction after the fishery is finished, as pearls often remain there mixed with the sand.

In spite of every care, tricks in picking out the pearls from the oysters can hardly be prevented. In this the natives are extremely dexterous. The following is one mode they put in practice to effect their purpose: when a boat-owner employs a number of hired people to collect pearls, he places over them an inspector of his own, in whom he can confide; these hirelings previously agree that one of them shall play the part of a thief, and bear the punishment, to give his comrades an opportunity of pilfering. If one of the gang happens to meet with a large pearl, he makes a sign to his accomplice, who instantly conveys away one of small value, purposely, in such a manner as to attract notice. On this the inspector and the rest of the men take the pearl from him: he is then punished, and turned out of their company. In the mean time, while he is making a dreadful uproar, the real thief secures the valuable pearl, and afterwards the booty is shared with him who suffered for them all. Besides tricks like these, the boat-owners and purchasers often lose many of the best pearls while the dony is returning from the bank; for, as long as the animal is alive, and untouched, the shells are frequently open near an inch; and if any of them contain a large pearl, it is easily discovered, and taken out by means of a small piece of stiff grass, or bit of stick, without hurting the pearl fish. In this practice they are extremely expert. Some of them were discovered whilst I was there, and received their due punishment.

Gmelin asks, if the animal of the *mytilus margaritiferus* is an *ascidia*? See Linn. Syst. Nat. tom. I. p. vi. 3350. This induces me to believe that it has never yet been accurately described; it does not resemble the *ascidia* of Linnæus, and may, perhaps, form a new genus. It is fastened to the upper and lower shells by two white flat pieces of muscular substance, which are called by Houttuin \* *ears*, and extend

\* Vide Houtt. Nat. Hist. Vol. I. p. xv, p. 381, seq.

about two inches from the thick part of the body, growing gradually thinner. The extremity of each ear lies loose, and is surrounded by a double brown fringed line. These lie almost the third of an inch from the outer part of the shell, and are continually moved by the animal. Next to these, above and below, are situated two other double fringed moveable substances, like the branchiæ of a fish. These ears and fringes are joined to a cylindrical piece of flesh of the size of a man's thumb, which is harder and of a more muscular nature than the rest of the body. It lies about the centre of the shells, and is firmly attached to the middle of each. This, in fact, is that part of the pearl fish which serves to open and shut the shells. Where this column is fastened, we find on the flesh deep impressions, and on the shell various nodes of round or oblong forms, like imperfect pearls. Between this part and the hinge (*cardo*) lies the principal body of the animal, separated from the rest, and shaped like a bag. The mouth is near the hinge of the shell, enveloped in a veil, and has a double flap or lip on each side; from thence we observe the throat (*œsophagus*) descending like a thread to the stomach. Close to the mouth there is a curved brownish tongue, half an inch in length, with an obtuse point; on the concave side of this descends a furrow, which the animal opens and shuts, and probably uses to convey food to its mouth\*. Near its middle are two blueish spots, which seem to be the eyes. In a pretty deep hole, near the base of the tongue, lies the beard (*byssus*), fastened by two fleshy roots, and consisting of almost one hundred fibres, each an inch long, of a dark green colour, with a metallic lustre;

\* The depth at which the pearl fish generally is to be found, hindered me from paying any attention to the locomotive power, which I have not the least doubt it possesses, using for this purpose its tongue. This conjecture is strengthened by the accurate observations made on muscles by the celebrated Reaumur, in which he found that this body serves them as a leg or arm, to move from one place to another. Though the divers are very ignorant with regard to the œconomy of the pearl fish, this changing of habitation has been long since observed by them. They allege, that it alters its abode, when disturbed by an enemy, or in search of food. In the former case they say it commonly descends from the summit of the bark to its declivity.

they

they are undivided, parallel, and flattened. In general, the *byssus* is more than three quarters of an inch without the cleft (*rima*); but if the animal is disturbed, it contracts it considerably. The top of each of these threads terminates in a circular gland or head, like the *stigma* of many plants. With this *byssus* they fasten themselves to rocks, corals, and other solid bodies; by it the young pearl fish cling to the old ones, and with it the animal procures its food, by extending and contracting it at pleasure. Small shell fish, on which they partly live, are often found clinging to the former. The stomach lies close to the root of the beard, and has, on its lower side, a protracted obtuse point. Above the stomach are two small red bodies, like lungs; and from the stomach goes a long channel or gut, which takes a circuit round the muscular column above mentioned, and ends in the anus, which lies opposite to the mouth, and is covered with a small thin leaf, like a flap. Though the natives pretend to distinguish the sexes by the appearance of the shell, I could not find any genitalia. The large flat ones they call males, and those that are thick, concave, and vaulted, they call females, or *pedoo-chippy*; but, on a close inspection, I could not observe any visible sexual difference.

It is remarkable that some of these animals are as red as blood, and that the inside of the shell has the same colour, with the usual pearly lustre; though my servants found a reddish pearl in an oyster of this colour, yet such an event is very rare. The divers attribute this redness to the sickness of the pearl fish, though it is most probable that they had it from their first existence. In the shade they will live twenty-four hours after being taken out of the water. This animal is eaten by the lower class of Indians, either fresh in their curries, or cured by drying; in which state they are exported to the coast; though I do not think them by any means palatable.

Within a mother-of-pearl shell I found thirteen *murices nudati* (*vide* Chemnitz's New System, Cabt. Vol. XI. tab. 192, f. 1851 and 1852), the largest of which was three quarters of an inch long; but, as many of them were putrid, and the pearl fish itself dead, I could not ascertain whether they had

crept in as enemies, or were drawn in by the animal itself. At any rate turtles and crabs are inimical to the animals, and a small living crab was found in one of them.

The pearls are only in the softer part of the animal, and never in that firm muscular column above mentioned. We find them in general near the earth, and on both sides of the mouth. The natives entertain the same foolish opinion concerning the formation of the pearl which the ancients did: they suppose them formed from dew-drops in connection with sun-beams. A Brâhmen informed me that it was recorded in one of his Sanscrit books, that the pearls are formed in the month of May at the appearance of the Soatee star (one of their twenty-seven constellations), when the oysters come up to the surface of the water to catch the drops of rain. One of the most celebrated conchologists\* supposes that the pearl is formed by the oyster in order to defend itself from the attacks of the *pholades* and *boreworms*. But we may be assured that in this supposition he is mistaken; for, although these animals often penetrate the outer layers of the pearl shell, and there occasion hollow nodes, yet, on examination, it will be found that they are never able to pierce the firm layer with which the inside of the shell is lined. How can the pearls be formed as a defence against exterior worms, when, even on shells that contain them, no worm-holes are to be seen? It is therefore more probable these worms take up their habitations in the nodes in order to protect themselves from the attacks of an enemy, than that they are capable of preying on an animal so well defended as the pearl fish is. It is unnecessary to repeat the various opinions and hypotheses of other modern authors; it is much easier to criticise them, than to substitute in their place a more rational theory. That of Reaumur, mentioned in the memoirs of the French Academy for 1712, is the most probable, *viz.* that the pearls are formed like bezoars and other stones in different animals, and are apparently the effects of a disease. In short, it is very evident that the pearl is formed by an extravasation of a glutinous juice, either within the body, or on the surface of the animal: the former case is the most com-

\* The Rev. Mr. Chemnitz at Copenhagen.

man. Between one and two hundred pearls have been found within one oyster. Such extravasations may be caused by heterogeneous bodies, such as sand, coming in with the food, which the animal, to prevent disagreeable friction, covers with its glutinous matter, and which, as it is successively secreted, forms many regular lamellæ in the manner of the coats of an onion, or like different strata of bezoars, only much thinner; this is probable, for if we cut through the centre of a pearl, we often find a foreign particle, which ought to be considered as the nucleus, or primary cause of its formation. The loose pearls may originally have been produced within the body, and, on their increase, may have separated and fallen into the cavity of the shell. Those compact ones, fixed to the shells, seem to be produced by similar extravasation occasioned by the friction of some roughness on the inside of the shell. These and the pearl-like nodes have a different aspect from the pearls, and are of a darker and bluer colour. In one of the former I found a pretty large, true, oval pearl, of a very clear water; while the node itself was of a dark blueish colour. The yellow or gold-coloured pearl, is the most esteemed by the natives; some have a bright red lustre; others are grey, or blackish, without any shining appearance, and of no value. Sometimes, when the grey lamella of a pearl is taken off, under it is found a beautiful genuine one; but it oftener happens that, after having separated the first coat, you find a worthless impure pearl. I tried several of them, taking one lamella off after another, and found clear and impure by turns; and in an impure pearl I met with one of a clear water, though in the centre of all I found a foreign particle. The largest and most perfect pearl which I saw during my stay at Condatchey was about the size of a small pistol bullet, though I have been told since my departure many others of the same size have been found. The spotted and irregular ones are sold cheap, and are chiefly used by the native physicians as an ingredient in their medicines.

We may judge with greater or lesser probability, by the appearance of the pearl shell, whether they contain pearls or not. Those that have a thick calcareous crust upon them,

to which *serpulæ* (sea tubes) *Tubuli marini irregulariter intorti*, *Crista-gali Chamar lazuras*, *Lepas tintinabulum*, *Madreporee*, *Millipore*, *Cellipore*, *Gorgontæ*, *Spongiæ*, and other Zoophytes are fastened, have arrived at their full growth, and commonly contain the best pearls; but those that appear smooth, contain either none, or small ones only.

Were a naturalist to make an excursion for a few months to Manâr, the small island near Jafna, and the adjacent coast, he would discover many natural curiosities still buried in obscurity, or that have never been accurately described.

Indeed no place in the East Indies abounds more with rare shells than these; for there they remain undisturbed, by being sheltered from turbulent seas, and the fury of the surf. I will just name a few of them; viz. *Tellina foliaca* Linn \*, *Tell Spenglerii*, *Arca culculata* †, *Arca Noæ*, *solen anatinus* Linn. *Ostrea Ifognomum*, *Terebullum*, *albidum*, *striatum*, *Turbo scalaris* ‡, *Bula volva* Linn. §, *Vexillum ingritarum*, &c. Amongst the beautiful cone shells; *conus thalassiar-chus Anglicanus cullatus* ||, *amadis thassiar-chus con. generalcis* Linn. *c. capitaneus* ¶, *c. miles* \*\*, *c. stercus muscarum* ††, *c. reteaureum*, *c. glaucus* ‡‡, *c. cereola*, *regia corona*, *murus lapidius*, *cauda erminea*, *societas cordium*. There are many others besides those already mentioned, equally valuable and curious.

The great success of the Rev. Doctor John in conchology when at Tutucorin, and assisted by G. Angelbeck, with a boat and divers; and the capital collections made by his agents, whom he afterwards sent there with the necessary instructions and apparatus, may be seen in Chemnitz's elegant Cabinet of Shells in 4to, (with illuminated plates); and how many new species of Zoophytes he discovered, we learn from another German work by Esper at Erlangen, the third volume of which is nearly finished.

\* The golden tong.      † Mounkscape.      ‡ Royal staircase.

§ Weaver's shuttle.      || Red English admiral.

¶ Green stamper.      \*\* Garter stamper.

†† Great sand stamper.      ‡‡ Caps. Gottw.



VII. *Account of a new Method of Bleaching Cotton.* By  
C. CHAPTAL, *Member of the National Institute* \*.

THE happy applications which C. Berthollet has made of the oxygenated muriatic acid, in the bleaching of stuffs manufactured from vegetable substances, seems to have carried this art very near to perfection; but this method is not every where attended with the same degree of economy. Besides, the process requires very skilful hands, that the stuffs may not be destroyed by too corrosive leys, or leys improperly employed; and therefore we ought not to omit making known other processes, in order that manufacturers may choose those which they may think most beneficial. I shall therefore here describe a process, both simple and economical, for bleaching cotton yarn.

At the distance of about one foot four inches from the grate of a common furnace, place a copper kettle of a round form, one foot and a half in depth and four feet in diameter, and fix it in that position. The brim of this kettle, about six inches in breadth, being bent outwards, will rest on the lateral edges of the mason-work of the furnace. The remainder of the furnace must be constructed of cut stone, in the form of an oval boiler, six feet in height, and in breadth, measured from the centre, five feet. The upper part of the furnace has a round aperture, in diameter about a foot and a half. This aperture may be closed by means of a moveable stone, or a copper lid made for the purpose. On the edge of the copper kettle, which forms the bottom of this species of digester, place a kind of grate consisting of wooden bars, brought pretty near to each other in order that the cotton laid upon them may not fall through, and sufficiently strong to sustain the weight of about 1600 pounds. When this apparatus has been constructed, impregnate the cotton disposed in bundles, with a slight solution of soda, rendered caustic by lime: this operation must be performed in a wooden or stone trough, in which the cotton may be trod

\* From the *Bulletin des Sciences*, Vol. II. No. 6.

upon by securing the feet with wooden shoes. When the alkaline liquor has well penetrated the cotton in an equal manner, it is to be carried to the boiler, and placed on the wooden grate already mentioned. The superfluous liquor will run down between the bars, and form a liquid stratum, which will permit the mass to be heated without any danger of burning the cotton or the bottom of the boiler. To form the alkaline ley, soda of Alicant may be employed equal to a tenth part of the weight of the cotton on which you operate; and in a boiler such as I have described, about 800 pounds of cotton may be subjected to the process at one time. At the moment when the cotton is introduced and arranged in the boiler, the aperture at top is shut, by its usual covering, as closely as possible, in order that the vapours may assume a greater degree of heat, and re-act with more force on the cotton. The fire of the furnace is then to be kindled \*, and the ley must be kept in a state of slight ebullition for from twenty to thirty-six hours. It is then suffered to cool, and the cotton being carefully washed, must be exposed on the grafs for two or three days, extending it on poles in the day-time, and spreading it out on the grafs during the night. The cotton will then have acquired a superb degree of whiteness; and if, by chance, any parts of the cotton are still coloured, it must be put into the kettle a second time, or be exposed a few days longer on the grafs. These shades in the bleached cotton are owing in particular to its not having been all equally and completely impregnated with the ley; they may arise also from the arrangement of the cotton in the boiler, if it has been heaped up too much in certain points. When it is judged that the ley has been exhausted by ebullition, the cover of the kettle is to be taken off, and the dry cotton must be besprinkled with a new solution of soda; without this precaution it might run the danger of being burnt. It might readily be judged, by estimating the substances and time employed in this operation, that it is attended with economical advantages; but we have a more simple method of as-

\* In giving the above dimensions, I have supposed the fuel used to be coal: if wood is burnt, the dimensions must be varied. In the latter case the bottom of the kettle would be too high above the bottom of the furnace.

certaining the fact, that is, the low price at which cotton is bleached in all those manufactories where this process is used. In the south of France, where this method is at present pretty generally adopted, eighty pounds of cotton are bleached for about seven shillings sterling. This process was brought to us from the Levant some time after the introduction of the process for dyeing the Adrianople red. It has been practised for some time, but kept a secret till the present moment, and still known by the name of *blanchiment à la fumée*, bleaching by smoke.

I do not know that this method has been applied to the bleaching of spun flax or hemp. It would, however, be worth while to try the experiment: stronger leys and longer ebullition, no doubt, must be employed; but it is by experience alone that we can acquire information on the subject.

VIII. *On the Method of preparing Inks that will withstand the Action of the Oxygenated Muriatic Acid.* By A. BOSSE, of Hamburg\*.

INTRODUCTION.

FOR illustrating the history of this subject, which has been fully treated of in the New Hanoverian Magazine, the following information may be necessary:—Dr. Lentin, in his paper containing cautions in regard to the misapplication of the oxygenated muriatic acid †, observed, that with the help of this acid our common ink could be obliterated without the least injury to the paper which had been written with it, if the paper were first drawn through diluted oxygenated muriatic acid; then through diluted sulphurous acid; and lastly, through water. At the same time he added some information which seemed to prove that this property had been employed in France several times to answer private purposes, and to the prejudice of others. Soon after appeared a

\* From *Scherer's Allgemeines Journal der Chemie*, Vol. II. No. 10. The Introduction is by Dr. Scherer.

† Hannov. Mag. 1797, part 71.

paper by Philip Christian Pitel of Minden \*, in which he recommended an ink, discovered by him, which was indestructible, and could be obliterated neither by the oxygenated muriatic acid, nor by any other corrosive substance. This induced M. Wehrs to examine this ink, and the same thing was undertaken by M. Gruner †. According to their experiments, this ink, however, was obliterated in the course of nine hours by the oxygenated muriatic acid, and also by caustic pot-ash; but at the same time an opinion entirely opposite was announced by Dr. Lentin ‡, M. Thorey §, and M. Wiegleb ||, who all, in consequence of their having employed the same test, declared the ink to be indestructible. These different opinions are therefore directly contrary to each other. M. Gruner found that the ink could be destroyed by the oxygenated muriatic acid, and by caustic alkali: M. Thorey observed, that its blackness was only lessened by the oxygenated muriatic acid, and by the vapour of that acid. He found, however, as M. Gruner did, that the caustic alkali dissolved the ink entirely from the paper, but that, like several acids, it destroyed the paper. M. Lentin and Wiegleb deduced from their experiments, those of the former being made with oxygenated muriatic acid, and those of the latter with the same, as well as several other acids, and even caustic alkali, that this ink was entirely indestructible. These contradictory results M. Westrumb endeavoured to resolve in his examination of Pitel's indestructible ink, which he found to be only common ink mixed with indigo; and he proposed the following mixture for an ink which could not be destroyed:—Boil 1 oz. of Brasil wood, and 3 ozs. of pulverised galls, with 46 oz. of water; strain the liquor, which must be boiled down to 32 ozs., and pour it, still warm, over  $1\frac{1}{2}$  oz. of perfectly pure sulphate of iron,  $1\frac{1}{4}$  oz. of gum arabic, and  $\frac{1}{4}$  oz. refined sugar. When these ingredients are dissolved, add from 1 to  $1\frac{1}{4}$  oz. good indigo ground exceedingly fine, and  $\frac{3}{4}$  oz. of purified lamp black.

\* Hannov. Mag. 1797, part 77.

† Ibid.

‡ Ibid. p. 1223.

§ Ibid. 1797, part 97.

|| Reich's Anzeiger 1797, No. 297.

## M. BOSSE'S PAPER.

It is well known that M. Pitel, of Minden, gave the first account of a kind of ink which withstood the oxygenated muriatic acid, at the desire of Dr. Lentin, who excited the attention of the public to various deceptions practised at Paris with common ink by means of the oxygenated muriatic acid. As this ink consisted merely of a decoction of logwood and galls with water, in which sulphate of iron, gum arabic, and fugar had been dissolved, and with which indigo and lamp black had been mixed mechanically, according to the analysis of M. Westrumb, it may be easily explained why it exhibited such different appearances when brought to the test. M. Gruner, apothecary in Hanover, as appears by his letter to the editor of the Hanoverian Magazine, was able to obliterate it entirely by the muriatic acid; while, on the other hand, its indestructibility by that acid is confirmed by M. Wiegleb and Thorey. Both are in the right: as soon as the ink is well stirred round, it withstands this acid; but, if not stirred, this acid dissolves the black oxyd of iron, and the paper in the places wrote upon is restored to its pristine state. Now, though this ink, when stirred round, does not entirely fail of its object, this circumstance, however, may be often forgotten; and this the more readily, as even without stirring it has all the blackness when used of common ink, on account of the oxyd of iron which it contains. Since that time I have made experiments in order to produce an ink which might withstand the oxy-muriatic acid, and yet contain no iron. I have been able to accomplish this, as well as the composition of some other inks which contain iron, though it does not make their chief component parts, but serves only to give the ink a black colour. I made experiments also with the juice of green plants, according to the process by which M. Murray made an indestructible kind of ink from the same substances. Iron must not be the chief component part of an ink destined to withstand the muriatic acid, because in the state of a black oxyd it is easily dissolved by the acid. But as it acts on vegetable colours only when they are capable of taking up oxygen, and does not destroy

them,

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them, but produces by its action a modification of the colour, they are exceedingly proper for this purpose. As none of these, however, were found entirely black, I was obliged to employ some mineral body which might be black and yet hold a great deal of oxygen. The most part of the metallic oxyds of a black colour contain very little oxygen, except that of manganese, which contains a great deal. I made choice therefore of this oxyd, and found, after repeated experiments, that it answered the intended purpose.

The oxy-muriatic acid dissolves all metals in a metallic form as well as metallic calces combined with a small quantity of oxygen, but not those which contain a great deal, as it has itself an excess of it. It is therefore capable,

1. Of dissolving metals in a metallic state, as the metals take up its superfluous oxygen, by which, as is well known, they become soluble in acids.

2. Such metallic oxyds as contain little oxygen are capable of abstracting it from acids, and they then dissolve in the acids, which then contain less oxygen.

The grounds which induced me to employ oxyd of manganese for preparing an ink capable of withstanding the oxy-muriatic acid, were as follows:—

1. Its black colour; 2. its containing a great quantity of oxygen, by which it is insoluble in the muriatic acid; 3. because it possesses so great an affinity for oxygen, that whenever it has lost any of its original quantity by being brought to a red heat, or exposed to the action of acids, it immediately draws it from the atmosphere, and again becomes black. I shall now describe the method of preparing this ink:—Boil 1 oz. of Brazil wood with 12 ozs. of water for a quarter of an hour; add  $\frac{1}{2}$  oz. of alum: evaporate the whole to 8 ozs., and mix with the liquor 1 oz. of exceedingly soft, finely pulverised manganese, mixed up with  $\frac{1}{2}$  oz. of pulverised gum arabic.

Brazil wood alone, by mere boiling, gives an ink not altogether unfit for use. Acids, less abundant in oxygen, such as the nitrous and muriatic acid, naturally exercise an action on this ink; but as these change the paper in a perceptible manner, deception is not to be apprehended.

Indigo also affords an ink that withstands the muriatic acid:

acid: it therefore makes a chief component part of that prepared by Pitel, but it is mixed in it only mechanically. The following is a prescription for preparing an ink of the like kind, but in which the indigo is actually dissolved.

Boil 1 oz. of Brazil wood, and 3 ozs. of coarsely pulverised galls, with 9 ozs. of vinegar and as much water, for the space of eight minutes: in the liquor, after being strained, dissolve  $1\frac{1}{2}$  oz. of sulphat of iron and 1 oz. of gum arabic, and then add to the whole a solution of  $\frac{1}{2}$  oz. of indigo in 1 oz. of concentrated sulphuric acid.

The oxy-muriatic acid dissolves the oxyd of iron in this ink, but the indigo remains undecomposed. The principal point to be attended to is, that the ink may contain such matters as are not affected by the oxy-muriatic acid: with other substances less deception is to be dreaded.

In the last place, I shall mention an ink made with the principal ingredients of common ink, but in preparing which, instead of the usual liquids, I employed the expressed juice of some plant: the fittest for this purpose I found to be the leaves of the caper spurge, *Euphorbia lathyris* LINN.; the common holly, *Sambucus niger*, and common grass.

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IX. *Sketch of the History of Mining in Devon and Cornwall.*

By Mr. JOHN TAYLOR jun. Miner, Tavistock. Communicated by the Author.

IT must be regretted that a subject of so great national importance, undertakings which engage so large a capital, and give employment to so many hands, as working the various mines in these kingdoms, should gain but little attention from any but those immediately concerned in them, and near the places in which they are situated; it is the more to be wished that men of science should devote some thought to these concerns, as those, under whose management most of these works are carried on, are often not men of sufficient ability or science to strike out capital improvements. To be a good miner requires an active mind, with industry and strict observation; these should be accompanied by some  
general

general knowledge, at least, of practical mineralogy, chemistry, mechanics, hydraulics, &c., and such a knowledge of principles as might lead to improvements in the practical part of his business. It would be unreasonable to expect to find these qualifications general at present, but the existing ignorance might surely be soon removed, if, the attention of men of science being turned to the subject, they would take pains to point out to the practical miner the improvements of which his operations are susceptible.

If it be true, as was asserted in the number of the Philosophical Magazine for February last, "that the poorest mine in Cornwall is worked at a greater expence than the richest one on the Continent," much is to be done; although to work a mine rich or poor to the same extent must require the same expence. From my own experience I cannot assent to what is asserted, in the same paragraph, of the captains or directors of the Cornish mines; I can bear testimony to there being among them men of ability, observation, and liberality; of all the present practical knowledge of the subject many of them are perfect masters, and have not often, I think, that desire of excluding interference which is imputed to them.

Mining is a subject requiring study, and deserving the attention of the philosopher, as it tends to unveil some of the most hidden processes of Nature, and to answer important ends to Society. On the Continent many of the most eminent men have not thought it beneath them to undertake the management of mines; and to this it is fair to impute the capital manner in which some foreign mines are reported to be conducted.

It must afford pleasure to know that of late years many improvements in mining, as well as in most other arts connected with or depending on the sciences, have taken place; and both the discovery of the ores, and their treatment after being brought to the surface, is, in most respects, carried on with greater certainty and skill, and consequently more profit.

It may afford some amusement to attend a little to the history of this source of the most ancient traffic of our island, and the cause, probably, of much of our civilization, it having  
attracted,



attracted, long ago, the visits of other more enlightened nations.

The tin of Britain was known in distant parts of the world at a very remote period. It is generally believed that the Phœnicians were the nation principally engaged in trading with Britain for this article. Tin works were certainly carried on before iron was in use in England. Many tools of oak are now found which tradition among the tinnerns make to have belonged to the Saxons or Danes; but it is probable, for the reason before stated, that they were employed before the time of their having a footing in this country.

The greatest quantity of tin was formerly, it is asserted, found on the forest of Dartmoor, in the county of Devon; and works to a vast extent must have been carried on there, if it be true, as we are told, that thirty thousand men found employment upon this tract; for all the miners now in Cornwall do not amount to more than ten thousand. It is, however, to be considered, that in the ancient mode of working, without the aid of machinery, many more hands must have been necessary than at present—I should think more than in the proportion of three to one. All these works on Dartmoor were only on or near the surface: now, however, the lodes\* or veins of tin found on this district are not valuable enough, or do not continue to such a depth as to make them very profitable to work.

Almost all the tin procured in former ages was probably from stream-works, in bottoms or low grounds, where fragments of the ore, washed from the lodes in the neighbouring hills, subside, and are separated from the earth in a granulated form by washing. This, of course, is obtained without any subterranean work. In such situations as these probably metals were first discovered, mixed merely with the upper soil, or lodging in clefts of rocks. Thus is gold found in America, in Africa, in the county of Wicklow in Ireland, and so has it been occasionally discovered in Cornwall; and veins or lodes of this metal might doubtless be found near the places where such depositions are met with.

The tracing and following lodes of ores into the earth is a

\* The name given in Cornwall to veins in which ore is found.

more difficult task than coming at the metal in the way of stream-works; it requires more energy of mind, and a more advanced state of the arts. The difficulty that must attend keeping a mine, sunk to some depth, free from water by manual labour only, could not but prevent men, before the application of machinery, from diving deep into the bowels of the earth. We have, however, some instances where old workings are found at so great a depth as to be even now with difficulty kept dry by means of machinery; but these, though they may be counted ancient, were probably opened long subsequent to the origin of mining in this county.

There are many mines which could not possibly be worked without the aid of gunpowder, and, until the discovery of this powerful agent, underground operations must have been uncertain and difficult. The hammer and wedges of metal were probably the first instruments for splitting rocks (and they still continue, in the ground that will yield to them, to be much used in Cornwall), and the pick, or instrument for cleaving the ground, having a head for driving the wedges called by the miners *gads*, from a Cornish word *gedn*, a wedge. The form of these instruments found in old works, I think, offers an evidence of their antiquity. A pick, which was found in Wheal\* Unity tin mine, in Devon, in a part not worked certainly for more than eighty years, and which could not probably be reckoned to be less than 100 years old, does not differ materially from the form of that now used; while one discovered in old workings in Drake Walls mine, in the parish of Calstock in Cornwall, about ten miles distant from the other mine, is of so different a shape as to make one conclude, judging by the slow progression of changes effected on common instruments, that it is of much higher antiquity. Wedges of dry wood were also very ingeniously made use of, by driving them into the clefts, and then wetting them, so as to cause them to swell and force the ground asunder. Fire was an agent long ago employed for splitting the rocks; but the effects of gunpowder so far exceed any thing before made use of for such purposes, that its dis-

\* In this part of the country the word *wheal*, signifying in the Cornish language *a work*, is generally prefixed to the proper names of mines.

covery and application to works of this kind form a grand epoch in the history of mining. This, it appears, took place in Hungary or Germany about the year 1620, and was first introduced into England at the copper mine at Ecton, in Staffordshire, about the year 1670, by German miners, brought over by Prince Rupert; but it was not in use in Somersetshire till 1684, after which, probably, the Cornish miners became acquainted with this powerful assistant to their operations.

Tin was the first object of the Cornish miner's search, and I shall therefore first take notice of the history of the tin-works. It was probably first found, as I have observed, near the surface, and not in regular veins; almost all the low grounds in Cornwall, and I believe every bottom on Dartmoor forest in Devon, bear the marks of having been *streamed*. The Romans, probably, interested themselves in the working of the mines; indeed, one principal inducement to that people's turning their attention to this island, seems to have been the metals that were reported to them to be found here. The Saxons neglected these hidden treasures, but the Normans worked them to great advantage. From that time to the end of the reign of John the mines were not profitable, and mostly in the hands of the Jews. They revived in the time of his son Richard; but in the reign of Edward I. the Jews were banished the kingdom, and the mines neglected. Edmund, the eldest son of the king, and earl of Cornwall, however, made some important alterations in the regulations of the tin-works by a charter, which was confirmed by Edward I. in the latter part of his reign. Indeed it is from this time that the peculiar laws and privileges relating to the Stannaries are chiefly to be dated. Mining infringed in some instances on property, and caused disputes, besides requiring indulgences not general; and thus cases arose not cognisable by the common law. In this way a peculiar code, springing from custom, took its rise; and though this in some measure existed before, yet it was not till this period that it was confirmed by royal charter, and enforced by subsequent acts of parliament. It was by this charter that the bounding land to the purpose of tinners working on it, the duties to

the earl of Cornwall, and the coinages of tin, or stamping with the earl's seal, were first established. Before the reign of Edward I. tanners worked in the earl's land only, paying him a fifteenth part of what they got, and they were not at all permitted to dig in sanctuary ground, churches, mills, houses, gardens, and so on; and if in working under they chanced to subvert any house, or to damage a high-way, they were obliged to make it good. When it became an object to search throughout any place or person's lands, a court also to determine cases relating to the tin-works became necessary; and this, adjudging under the authority of, and according to the code of laws before mentioned, was first established by Edward I. and is called the Stannary Court.

In Cornwall and Devon two different systems of Stannary laws now exist: those in Cornwall have been from time to time amended and corrected; but in Devon, where mining has for a long while slumbered, the laws continue in their original crude state. As the spirit, however, of searching into the bowels of the earth seems again to be reviving in this county, the laws, should they be called into action, will probably undergo some revision.

The duke of Cornwall had royal jurisdiction, and when the duchy came into the hands of the crown, which happened in the reign of Edward III. he made his eldest son, the Black Prince, the duke, and appointed it, with the revenue arising from it, to the sons and heirs apparent of the crown, though no son of the king can be duke of Cornwall but the first-born, even though heir apparent.

The Stannary laws were explained and confirmed, with some alterations at different times, till Arthur, eldest son of Henry VIII. made certain constitutions relating to the Stannaries, which the tanners refused to observe: the king, after prince Arthur's death, seized the charter as forfeited, but granted a new one with fresh privileges, appointing all new laws relating to the tanners to be made by a parliament of their own body; and upon this footing the Stannaries remain.

Great powers have been at times assumed by this jurisdiction,

tion, it has even taken upon it the trial for felony in the case of a tinner; in general, however, the cases where its authority interferes, are in disputes concerning tin-works or between tinners: it provides a supply of water to a tin-work, giving a power to conduct it through any lands for that purpose.

One of the improvements, with regard to tin particularly, was the invention of smelting in reverberatory furnaces with pitcoal, instead of the old *blowing-houses*, as they were called, with charcoal fires. When this change was first introduced is not perhaps certainly known: Dr. Watson, in his *Chemical Essays*, states, that Becher resided in Cornwall some years before he died, which was in the year 1682; and that he made many improvements in the working of mines and fluxing of metals, and introduced there the method of smelting tin by the flame of pitcoal thrown upon the ore in a reverberatory furnace, instead of the fire of wood or charcoal, in the way formerly made use of. He takes notice of this in the Dedication to Mr. Boyle of his *Alphabetum Minerale*, written at Truro in 1682, not long before he died. Pryce, in his *Mineralogia Cornubiensis*, states, that this mode was first prompted by necessity, and many experiments were made upon it by Sir Bevil Granville, of Stow, in the time of Charles I. though it was not effectually done till the reign of Queen Anne.

I find, in the revision of the Stannary laws at the convocation of the parliament of Stannators assembled in the 26th year of the reign of George II. mention is made of the "ancient laws and constitutions of the Stannaries relating to tin-blowers, and refining of tin in *blowing-houses*, which was formerly the only method of refining of tin;" and it is said, "of late years there has been another method found out of refining of tin by means of reverberatory furnaces, which is commonly called smelting of tin."

Copper, of which so much is now sent from this part of the country, was not an object attended to, till a comparatively late date, by the Cornish miners: even in tin mines, which as they deepened produced copper, as is often the case, and where they needed to raise this ore, it was thrown by as of no value, going by the name of *poder*. Those who

live in the present more enlightened period, are now reaping profits left to them by the ignorance of their forefathers. This proves of what consequence it is to determine, if possible, the value of all the substances passing under the miner's observation.

Copper, however, was probably worked at a remote period in Wales, at the Parys mountain, which indeed is supposed to derive its name from the Celtic word *praas*, brass or precious metal; and this would offer a proof of its having been worked by the ancient Britons. It was not attended to, or at least not well understood, in England, till the reign of Queen Elizabeth, who paid great attention to the mines of the kingdom, and, by granting great privileges to Daniel Houghsetter, Christopher Schutz, and other Germans whom she invited into England, commenced and established the highly valuable and important business of finding and purifying this useful metal. To these foreigners, too, is owing the flourishing state of our brass manufactories. In this reign Parys mine was granted to patentees, but was not worked, at least to any advantage, for a century and a half.

The copper mines of Cornwall now are works of great magnitude, and some have been sunk to an amazing depth, and are kept working at a vast expence. The quantity of materials constantly used forms an extensive commerce to this part of the kingdom; and this, as well as the mines themselves, gives employment to numbers.

The other metals are not found in Cornwall in great abundance. Lead, with silver contained in the ore, is found in some places; and in Devonshire, on the borders of this county, a very large mine at Bere afforded galena very rich in silver. It was formerly worked as a royal mine, but afterwards lay idle a long time. It has since been feebly tried; but though a steam-engine was on it, they could not go so deep as it had been before sunk without more power to draw the water, and not finding much valuable return, the mine has been lately stopped again. Veins of lead ore sometimes run with copper lodes.

Iron is found in some places, but not in large quantities; and coals being distant, it will not repay the cost of procuring.

The ores of many of the semi-metals, as zinc, cobalt, arsenic, manganese, &c. are scattered in different places, and of late have received attention.

Mining has been much improved within this century. Till about a hundred years ago, the water was drawn from the mine by dint of human labour; but within these seventy years the application of hydraulic engines has become general. The introduction of the steam-engine formed a grand æra in the annals of mining; as in many situations no other power, that could be commanded, could possibly work the pumps necessary to raise the water from the bottoms: the consequence has been, that many mines have been worked that were formerly abandoned as impossible to be prosecuted. Other improvements of late years have been creeping in; the dressing of ores, or properly separating them from the earths, &c. in which they lie, is much better managed than formerly.

Great spirit is to be seen in the works in Cornwall; prodigious sums are laid out in erecting powerful machinery, sometimes even before any quantity of ore is seen. This often well repays those who expend it, especially if done under the direction of experienced and skilful managers, who are good judges of the appearances and symptoms on which is founded the expectation of finding a valuable return.

The very spirited and laudable exertions that are making, at a great risk, to bring to the public use so valuable an article as the copper mines produce, have lately been damped by shutting up the channel of trade with regard to this commodity. The purpose for which this is done, namely, a reduction in the price, cannot in the end be answered; for even should that for a time take place, the deep, and consequently expensive mines, and those which produce ores of inferior quality, will probably be stopped; and thus, a smaller quantity coming to market, the price must again advance, perhaps higher than it now is. In the mean time, should it operate to reduce the price, a valuable source of traffic will be lost to the nation; the revenue will suffer; individuals be distressed; and numbers of people not used to any other way of life become destitute of the means of existence.

X. *On the relative Proportions of Coals and Iron-Stones used at the Blast-Furnace, and of their proper Application to Use.* By Mr. DAVID MUSHET, of the Clyde Iron-Works. Communicated by the Author.

**I**N the smelting operation a just proportion and association of materials and mechanical construction ought to be blended in order to produce the best possible effects. Under the former are comprehended the cokes, iron-stone, limestone, and blast; by the latter is understood the furnace, the power of the blowing-machine, or the compression and velocity under which the air is discharged into the furnace, and the genius or mechanical skill of the workmen. According to this division I shall endeavour to point out the very various effects which disproportion in any case produces, and *vice versa*.

In the preceding papers the coal and iron-stone have been traced through their various stages of preparation, and that stage pointed out in which they were most suitable for the profitable manufacture of the metal. It will be necessary to carry along with us this fact, that in the exact proportion which the quantity of carbon bears to the quantity of metal in the ore, and its mixtures, so will be the fusibility, and of course the value of the pig-iron obtained. The importance of this truth will still farther appear when we consider the very various qualities of pit-coal, the different proportions of carbon which they contain, and the various properties attached to every species of this useful combustible.

Among the many strata of coal which I have distilled, some I have found to contain 70 parts in the 100. This large proportion is peculiar to the clod-coal, used at some of the iron-works in England, and justly preferred, for the purpose of manufacture, to the purest and hardest variety of splint-coal. The latter I have found to average from 50 to 59 parts of carbon in the 100; and the soft, or mixed qualities of coal, from 45 to 53 parts. Such various proportions of carbon plainly point out, that the operations to be followed at each individual iron-work ought not to rest upon



precedent, unless borrowed from those works where exactly the same quality of coal is used. This analysis also lays open part of the source from whence originates the widely different quantities of metal produced *per week* at various blast-furnaces, and the great disproportions of ore used to different coals.

Experience has shewn that the three qualities of coal just mentioned, will smelt and give carbonation to the following proportions of the same species of torrefied iron-stone:—

112 lb. of clod-coal cokes will smelt - - 130 lb.

112 lb. of splint-coal cokes will smelt - - 105 lb.

112 lb. mixed soft and hard coal cokes will smelt 84 lb.

Let the iron-stone be supposed in the blast-furnace to yield 40 *per cent.* then we find that the 1-20th of a ton of the respective qualities of cokes will smelt and carbonate the following proportions of iron, *viz.*—112 lb. clod-coal cokes, 130 lb. iron-stone, at 40 *per cent.* = 52 lb. iron; 112 lb. of splint-coal cokes, 105 lb. of the stone = 42 lb. of iron; and 112 lb. soft and hard coal cokes, 84 lb. of the iron-stone = 33  $\frac{6}{10}$  lb. of iron. We then have for the quantity of metal produced by one ton of each quality of cokes:

Clod-coal: 52  $\times$  20 = 1040 lb.

Splint ditto 42  $\times$  20 = 840 lb.

Mixed ditto 33  $\frac{6}{10}$   $\times$  20 = 702 lb.

This furnishes a datum whereby we easily obtain the quantity of the various cokes necessary to produce 1 ton of carbonated crude iron by common proportion: for if 1040 lb. of metal are produced by 1 ton, or 2240 lb. of clod-coal cokes, the quantity of the same cokes requisite for the production of 1 ton, or 2240 lb. of metal, will be: *T. C. 2. lb.*

1040 : 2240 :: 2240 : 4824.6 lb. = 2 3 0 8

Splint-coal cokes 840 : 2240 :: 2240 : 5973.3 lb. = 2 13 1 9

Mixed ditto 702 : 2240 :: 2240 : 7147.5 lb. = 3 3 3 7

If to the quantity of cokes necessary to manufacture 1 ton of crude iron, we add the quantity of volatile matter driven off in the process of charring, which may be thus estimated upon the average of each quality:—

Clod coal  $\frac{3}{8}$  or 37  $\frac{1}{2}$  *per c.* produce in cokes  $\frac{5}{8}$  a 62  $\frac{1}{2}$  *per c.*

Splint coal  $\frac{4}{8}$  — 50 —————  $\frac{4}{8}$  or 50

Mixed coal  $\frac{5}{8}$  — 62.5 —————  $\frac{5}{8}$  — 37  $\frac{1}{2}$

Then,

Then, for the quantity of the respective coals used in the raw state, we have the following results in proportion:—

					T. C.	Q.	lb.
Clod-coal	5:4824·6	:: 8 :	7719 $\frac{3}{4}$	=	3	8	2 19
Splint-coal	4:5973·3	:: 8 :	11946	=	5	6	2 18
Mixed coal	3:7147·1	:: 8 :	19158 $\frac{2}{3}$	=	8	11	0 16

These great disproportions of quantity, used to fabricate 1 ton, or 2240 averdupoise pounds of the same quality of crude iron, will convey a striking and impressive idea of the multifarious qualities of coal which may be applied and made to produce the same effects. It should also convince the manufacturer that the study and analysis of his own materials is the first and radical approach to true knowledge, and certainty of operation. Divest him of this knowledge, and view him guided by the *customs* and *rules* prevalent at another manufactory, where the coals and ores may be as different as has been already mentioned, and we will no longer wonder at the uncertainty of his results, and the numberless errors of his direction.

Before I enter into the practical discussion of the application of coal, I beg leave to indulge myself in the following calculations:—We have already seen that the production of 2240 lb. of carbonated crude iron requires 4824 lb. of clod-coal cokes; these may be averaged to contain 4·5 *per cent.* of ashes, which, deducted from 4824, gives 4607 lb. of carbon used for 1 ton of metal: this sum, divided by 2240, farther gives, for 1 lb. of cast iron thus manufactured, 2·056 lb. of carbon.

We next find that 2240 lb. of the same metal requires of splint-coal cokes 5973·3 lb.; we farther find, from a table of the analysis of coal, furnished in a former paper, that 100 parts of the raw coal contained 4·2 parts of ashes. As it is there stated to lose 50 *per cent.* in charring, 100 parts of cokes will contain 8·4 of ashes; and 8·4 *per cent.* deducted from 5973·3, gives 5472 lb. of carbon. This again, reduced by 2240 lb. gives for each lb. of metal manufactured, 2·442 lb.

Again, 7147·1 lb. of cokes produced from soft mixed coals are consumed for every ton of 2240 averdupoise pounds of crude iron produced; every 100 parts of the same coals contain

tain 3·3 parts of ashes; and 100 parts of cokes contain nearly 6·5 *per cent.* of ashes, which, deducted from 7147·3, gives 6672·6 of carbon, which divided by 2240, gives, for the quantity used for 1 lb. of cast iron, 2·978 lb.

From these calculations it appears, that 2240 lb. of carbonated iron, requires of carbon from clod-coal 4607 lb.; of carbon from splint-coal, 5472 lb.; and of carbon from mixed coal, 6672 lb.: that 1 lb. of carbonated iron requires of carbon from clod-coal cokes 2,056 lb.; from splint, 2,442 lb.; from mixed, 2,983 lb.: and that carbonated crude iron may be obtained when widely different quantities of carbon have been consumed.

In seeking for a solution of the latter fact, we must have recourse to the different degrees of inflammability of the carbon, according to the various laws of continuity imposed upon it in its fossil construction. It can easily be conceived, that, owing to this structure, and the nature of the interposed ashes, the particles of carbon of some cokes will be more easily oxygenated than those of others; in the same way that we find splint-coal, when exposed to ignition in contact with open air, affords 1-3d of more cokes than are obtained from soft mixed coals, though the latter, when distilled, yields more pure carbon than the former.

By experiment it is proven, that 100 grains of carbonic acid gas is composed of 72 parts of oxygen united with 28 parts of carbon: if the quantity of the carbon of clod-coal, *viz.* 2·056 lb. used for the manufacturing of every pound of cast iron, is reduced to grains, we will find it to consist of 14392 grains; this, divided by 28, gives the acidifiable principle of  $514 \times 100 = 51400$  grains of carbonic acid \*:

\* This is supposing, for the moment, that the whole of the carbon is oxygenated, either by the oxygen contained in the ore, or obtained from the discharging-pipe by the decomposition of the atmospheric air: this, however, is not strictly true, as the metal takes up a small portion, by weight, of the carbon; and when, by accident, moisture has been introduced into the furnace, either through the medium of the blast, or of the materials, its decomposition furnishes a portion of both oxygen and hydrogen, which may dissolve, and also carry off, a part of the carbon. Atmospheric air being found to hold water in solution, a small quantity of hydrogen will, even in the driest weather, be present in the blast-furnace. M.

hence, as 1 cubic foot of this gas, at 29.84 of barometrical pressure, and 54.5 of temperature, weighs nearly 761 grains, we find, that in the formation of every pound of cast iron

$$\frac{51400}{761} = 67,54 \text{ cubical feet of carbonic acid gas will be}$$

formed; and in the production of 1 ton of metal, the astonishing quantity of 151289,60 cubic feet. This quantity, however incredible it may seem, is only what would be formed under the above pressure, and at the above temperature: when we take into the account the high temperature at which the decomposition and recombination are effected, with the consequent increase of elastic force and of volume, our ideas are almost unable to commensurate the sum of the gas hourly formed, and thrown off, ignited to the highest degree of heat.

If the same mode of calculation is adopted with the other qualities of coal, we will have the following results:—

For the splint-coal 2,442 lb. or  $\frac{17094}{28} = 610,5 \times 100$   
 $= 61050$  grains of carbonic acid, which gives  $\frac{61050}{761} =$   
 $82,85$  cub. feet for 1 lb., and  $82,85 \times 2240 = 185,584$  cub.  
 feet for 1 ton. For the mixed coal 2,983 or  $\frac{20881}{28} = 710 \times$   
 $100 = 71000$  grains carbonic acid; that is,  $\frac{71000}{761} = 93,3$   
 cubical feet for 1 lb.; and  $93,3 \times 2240 = 208,992$  cubical  
 feet for 1 ton. By the same calculation we may attain a  
 pretty accurate notion of the quantity of atmospheric air ne-  
 cessary to produce 1 lb. or 1 ton of cast iron; an average of  
 the three varieties of coal will be sufficiently accurate for this  
 this purpose; thus  $\frac{14392 + 17094 + 20881}{3} = 17455\frac{2}{3}$  or  
 2,4935 lb. of carbon are consumed upon the average of each  
 pound of pig-iron: this is found to produce of carbonic acid  
 gas  $\frac{17455\frac{2}{3}}{28} = 62,341 \times 100 = 62,30041$  grains; which  
 again divided by 761, the grains in one cubic foot gives 81.86  
 cubic feet for the gas discharged in manufacturing one pound

of cast iron. As carbonic acid contains, as has already been noticed, 72 parts of oxygen in 100, then we have for the quantity of oxygen gas  $100 : 72 :: 62300.41 : 44856.29$  grains oxygen gas; and as, at the ordinary temperature and pressure of the atmosphere, a cubic foot of oxygen gas weighs 591 grains, we find  $44856.29$  divided by  $591 = 75.89$  cubic feet of oxygen gas necessary to form the acidifying principle of  $81.86$  cubic feet of carbonic acid gas; and that the same quantity of oxygen gas is necessary to the production of one pound of carbonated crude iron. This leads us to the following statement for the quantity of atmospheric air used during the same operation; first premising that the constituent parts of atmospheric air are nearly 73 of azote and 27 of oxygen gas; of atmospheric air then necessary, we have  $27 : 100 :: 75.89 : 281$  cubic feet.

I shall now proceed from mere calculation to matter of fact, and attempt to prove the correctness of the former by the approximation of the latter to its results. Let a blast-furnace be supposed to produce  $20\frac{1}{4}$  tons of pig-iron *per week*,  $= 45360$  averdupoise pounds; this, divided by days, hours, minutes, and seconds, gives *per day* 6480 pounds, *per hour* 270, *per minute*  $4\frac{1}{2}$  lb., and *per second* 525 grains.

From this it is evident that 1 lb. of cast iron is produced in  $13\frac{33}{100}$  seconds: experience has shewn that a blast-furnace, producing, in any of the above periods, the respective quantity of metal, requires a discharge of air *per minute* nearly equal to 1350 cubic feet; this, divided by 4.5 lb., the quantity produced *per minute* gives, for 1 lb. iron, 300 cubic feet. The quantity, by calculation, we have seen to be 281 cubic feet—difference 19: a sum no way considerable when we reflect upon the inequality of the movements of a blowing machine, and when it is recollected that some allowance ought also to be made for what air may pass through the furnace undecomposed, or may be lost at the place of entrance.

From this coincidence of theory with practice, we cannot help admiring the rigorous principles on which the Lavoisierian system is founded; nor are we less pleased to find, that, small as the operations of the chemist may be, yet they are a

just epitome of what takes place in the philosophy of extensive manufactories. The following table exhibits the quantity of carbon which may be used upon an average, with the relative quantity of carbonic acid formed, and air used:—

In the manufacture of 1 lb. — 1 ton of iron,			
The pure carbon requisite is	2·49	—	5585·44 lb.
Carbonic acid formed	81·86	—	183366·40 cub. feet
Oxygen gas used	75·89	—	169993·60 cub. feet
Atmospheric air employed	281·00	—	629440·00 cub. feet.

From the foregoing particulars upon coal may be learned how much is dependent upon the native construction of coal and its constituent parts; I shall next advert to the effects produced by its improper preparation.

When coals intended for the blast-furnace are sufficiently charred, they ought, in point of colour, to be of a silver grey; their fracture will appear lamellated and porous if splint-coals have been used; softer coals form themselves into branches slightly curved, and, when properly prepared, are always very porous. I have frequently found that the better the cokes were charred, the more water they will absorb. Coals half burnt do not take up half so much water, because their fracture continues in part to be smooth and less porous than when thoroughly burnt.

When half-prepared cokes are introduced into the furnace, the metal formerly carbonated will lose its grey fracture, and approach to the quality of oxygenated iron. Their presence is easily detected by the unusual quantity of thick vapour arising along with the flame. Besides, the water and sulphur, which raw coals introduce into the furnace, and which always impair the quantity of carbon by the various solutions effected by the presence of oxygen, hydrogen, &c. the fitness of the coal for combustion, and the support of the ore, is much diminished by this second course of ignition and disengagement of bitumen. The pressure of the incumbent ores also fracture, and reduce the cokes into small pieces, which produce a considerable portion of coke-dust; this is partly carried to the top of the furnace before the blast: sometimes below it appears in immense quantities, ignited to whiteness, and liquid as sand. Coal thus detached from the mass, exposed

posed to the action of a compressed current of air, is unfit for conveying the carbonic principle to the metal; and as it frequently belongs to the just proportion of charcoal necessary to smelt the ores, and to carbonate their iron, its loss must be felt, and the quality of iron impaired.

When cokes of any quality are exposed to a moist atmosphere, so as to absorb water, their effects in the blast-furnace become much reduced, and the presence of the water is productive of the most hurtful consequences in the production of carbonated crude iron. I have found, by repeated experiment, that 1 lb. of well-prepared cokes will, when laid in water, take up  $1\frac{3}{4}$  ounces in the space of half an hour; at this rate, a basket of cokes weighing 80 lb. saturated with water, will contain 140 ounces of water, or  $8\frac{1}{2}$  lb. If the charge contains six baskets, then we see that upwards of 50 lb. of water is introduced regularly along with the charge, furnishing an additional quantity of oxygen equal to  $42\frac{1}{2}$  lb., and of hydrogen equal to  $7\frac{1}{2}$  lb.: it frequently happens that the cokes contain a larger portion of water than is here stated. When cokes thus furcharged are introduced in quantity into the blast-furnace, the quality of the metal is not always instantaneously changed, and frequently the colour and form of the cinder remain long without any great alteration. The contact of wetted cokes with the ore is first seen by the great discharge of pale-blue gas, with the whiter flame at the top of the furnace; next, the accumulating oxyde upon the surface of the pig when consolidating indicates their presence. Iron thus oxygenated frequently exhibits, while fluid, that agitation and delicate partings peculiar to carbonated metal: the remelting of this iron is never attended with advantage, and is always unprofitable to the founder.

From the properties assigned to pit-coal in this and in former papers, the following facts may be deduced:—That charcoal is the basis of the manufacture of crude iron; that its proper application produces the most valuable qualities of pig-iron; that, by diminishing its relative proportion, or contaminating its quality by heterogeneous mixtures, the value and fusibility of the metal is lost; but that, by a proper increase,

crease, and always in proportion to this increase will the fusibility and value of the iron be mended. From the whole an important lesson may be learned of the pernicious effects of water in the furnace, and how absolutely necessary it is to prepare the cokes without using water, either to damp the fires, as in the usual mode, or to cool the cinders obtained from the tar kilns, to prevent their consuming in the open air: in all this hurtful operation considerable quantities of water become fixed in the cokes, which require a very great degree of heat to expel.

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THE preparation of iron-stone has already been fully attended to, and the phenomena which it exhibits under every stage minutely described. In consequence of various experiments we are authorised to draw the following conclusions: That when pure calcareous iron-stone is used, it admits of having the local quantity of cokes diminished; that argillaceous requires a larger portion than the calcareous genus; and that siliceous iron-stone requires a greater proportion of fuel than any variety of the former genera. We have also seen that fusibility, either connected with strength or otherwise, is derived from the mixture of the ores; and that excessive brittleness, intimately connected with infusibility, is also derived from the same source. From a review of these facts, we are forcibly impressed with the importance of combining the prepared iron-stones with proportions of fuel suited to their various natures, in order to produce all the varieties of crude iron with the greatest possible œconomy. Contemplating farther the same subject, it is easy to be conceived that a want of knowledge of the component parts of iron-stones, and the effects which individually they produce, must lead to great uncertainty of operation in the smelting process, wherein the beautiful œconomy of nature, and even real property, will be often unprofitably sacrificed to precedent.

Besides the above causes of alteration, dependent upon mixtures of the earths, the existence of oxygen in various quantities in the ores ought never to be overlooked in proportioning the cokes to the iron-stone. This powerful agent,  
whose



whose form and substance constantly eludes our vision; whose existence is only ascertained by the wonderful changes produced by its various combinations with the iron; and whose presence in the same iron-stone, in various quantities, may produce such variety of result as to characterise the ores, as containing *good* or *bad* iron, surely forms the most interesting mixture which ores or iron-stones possess. It will be a momentous epoch in the manufacture of iron when the existence of such a principle shall be fully admitted by the manufacturer, and its agency, from certain visible effects produced, adopted to explain its accompanying phenomena. Till that period he will not perceive the utility of ascertaining the quantity of oxygen, and devising economical methods of taking it from the ore. An attention to this powerful principle can alone root out those prejudices so inimical to the real interests of the manufacturer, and which seem to glance at Nature, as having improvidently combined her most useful metal with mixtures which could resist the ingenuity of man, or set his comprehensive intellect at defiance. In the progress of this great inquiry, is it not possible that the present expensive exertions may in part be superseded? Is it not possible, that, by laying open the sources of information to individuals at large, a greater mass of intellect may engage in the practice of this art? While the present extensive and lofty buildings are necessary, the business is entirely confined in the hands of men of great capital: the extent of their manufactures require that a large tract of country be devoted to their supply; a natural consequence is, that innumerable small tracts of land are overlooked, or held unworthy of notice, merely because they cannot, in a period necessary to clear a great capital and insure a fortune, afford the necessary supply of materials. Such situations, according to the present state of the iron business, must remain unexplored. Should, however, a desire for truth once gain footing in the manufactories of iron, and should this natural impulse of the unprejudiced mind keep pace with other branches of intellectual information, we may not despair of seeing many imperfections removed, which were the unavoidable consequence of the period of their creation.

In the application of iron-stone in the blast-furnace, the following particulars ought rigorously to be attended to:—

1. Their mixtures, whether clay, lime, or filex: their relative proportions to each other, judging according to the rules formerly laid down; which of them may admit of a diminution of fuel; which of them will afford the quality of iron at the time requisite; and which of them will be most likely, by a judicious arrangement, to give the greatest produce of metal, united with value and œconomy. Iron-stones, united with large portions of filex, have already been stated to require a greater proportion of fuel to carbonate their metal than the other genera. When ballast or forge-pigs are wanted, it stands to reason that siliceous iron-stones ought to be used; not that they contain a greater quantity of iron, but because they form a substitute for the other kinds, which may be more advantageously smelted for the production of more valuable qualities.

2. The quantity of metal which each individual iron-stone may contain, is another object of consideration. Besides the proportion of mixtures, which chiefly contribute to the fusibility of iron-stones, a second degree of fusibility is dependent upon the richness of the ore in iron: this is so obvious in the use of the Cumberland and Lancashire ores, that the consequences of their introduction will be perceived, by the change of the scoria and metal, in half the time that change would be effected by ordinary iron-stones. It has been frequently noticed, that crude iron contained pure carbon in proportion to its fusibility; then the more fusible, or supercarbonated qualities, must take up, comparatively, a considerable portion of the carbonaceous principle from the fuel. From this results a striking consequence, that the quantity of fuel should, over and above its relation to the mixtures, bear a just proportion to the quantity of iron in the stone: for example, let the weight *per* charge of fuel at a blast-furnace be 400 lb., and let this be supposed sufficiently to fuse and carbonate the iron contained in 360 lb. of iron-stone; let the quantity of metal be supposed 35 *per cent.* then the produce will be 126 lb. Should a change take place, and iron-stone richer in iron be applied, though the same by weight, and should this iron-stone

stone yield of torrefied stone 45 *per cent.* its produce will be 162 lb. or 40 lb. more than the former. As there exists no greater proportion of carbon in the furnace, it is evident that the existing quantity, being distributed over nearly 1-3d of more metal, must therefore be in more sparing quantity in the whole, and the value of the metal consequently reduced.

3. The weight of oxygen contained in iron-stones is the next object of serious consideration. I have already shewn; from experiment, that our iron-stones naturally contain from 9 to 14 *per cent.* of oxygen, which remains after torrefaction; it has also been shewn, that this quantity of hurtful mixture may easily be doubled by over-roasting or under-roasting the stone; and that the bad effects entailed are in the ratio of its combination with the iron. From a review of the facts adduced on this subject in various parts of my papers, its agency and effects will easily be credited by men of science; its property of constituting the acidifying base of all the acids readily explains the unalienable consequence of its presence with acidifiable bases. The effects are still more pernicious when the oxygen is furnished by the decomposition of water in raw iron-stone; the hydrogen in this case set free, also seizes a portion of the carbon; and these abstractions, united to that produced by the native portion of oxygen in the stone, form an aggregate which frequently reduces the value of iron 40 *per cent.* So long as the principles of science are overlooked in the manipulations of the foundry and forge, the existence of such agents will be treated as chimeras of the philosopher and chemist, and the effects hourly produced by them industriously attributed to causes which in point of unity or consistency will not bear the slightest touch of investigation.

XI. *A new Theory, pointing out the Situation of the Magnetic Poles, and a Method of discovering the Longitude, By P. R. NUGENT, Esq. formerly Surveyor-General of Lands for the Island of Cape Breton. Communicated by the Author,*

THE several interesting and important voyages, undertaken by order of his present Majesty, for making discoveries highly beneficial to mankind, particularly in what relates to geography and navigation; and the great desire and intention of deriving every possible benefit from the many observations which such extended voyages were designed to offer ample room and opportunity for making, became objects of peculiar concern and consequence. In addition, therefore, to the undoubted and acknowledged abilities of the respective commanders and officers appointed to such designations, the Commissioners of the Board of Longitude thought fit to appoint, for every voyage, one or more persons on whose scientific ability, fidelity, and diligence in assisting to make the respective observations, they could also rest satisfied. These gentlemen were therefore furnished with the completest instruments which the most scrupulous care, intention, and circumspection, together with a total disregard to expence, could be found to procure. Their report and testimony is therefore conclusive proof of the perfection or imperfection of the several instruments they had with them; their report of the azimuth compass, and dipping-needle or compass, is as follows:—

*Of the Azimuth Compass.*

The late ingenious and accurate the Hon. Captain Phipps, afterwards Lord Mulgrave, in his account of his Voyage towards the North Pole, p. 108, remarks as follows:—"The variation of the compass, always an interesting object to navigators and philosophers, became peculiarly so in this voyage, from the near approach to the Pole: many of the theories that had been proposed on this subject were to be brought to the test of observations made in high latitudes, from which alone

alone their fallacy or utility could be discovered; they of course engaged much of my attention, and gave me the fullest opportunity of experiencing, with regret, the many imperfections of what is called the azimuth compass. This instrument, though sufficiently accurate to enable us to observe the variation so as to enable us to steer the ship without any material error, with the precaution of always using the same compass by which it is taken, is far from being of such a construction as to give the variation with that degree of precision which should attend experiments on which a theory is to be founded, or by which it is to be tried. The observations taken in this voyage will fully evince this by their great variations from one another in short intervals of time; nor is this disagreement of successive observations peculiar to high latitudes, and to be attributed to a near approach to the Pole, as I found it take place even upon the English coast."

Mr. Wales, F. R. S. Master of the Royal Mathematical School in Christ's Hospital, Secretary to the Board of Longitude, &c. in page 49 of the Introduction to the Original Astronomical Observations made in the Course of a Voyage towards the South Pole, and round the World, in his Majesty's ships the Resolution and Adventure, in 1772, 1773, 1774, and 1775, by himself and Mr. William Bayley, now Master of the Royal Academy at Portsmouth, published by order of the Board of Longitude, says:

"I cannot pass over this article without making a remark or two on the irregularities which we found in the observations made with these instruments in the Channel of England: the extremes of the observed variations were from  $19^{\circ}$  to  $25^{\circ}$ ; and all the way from England to the Cape of Good Hope I frequently observed differences nearly as great without being any way able to account for them, the difference in situation being by no means sufficient. These irregularities continued after leaving the Cape," *i. e.* they continued throughout the voyage. But these irregularities are trifling compared with those

#### *Of the Dipping Needles, or Compasses.*

These instruments were so imperfect in principle, and the observations made with them so uncertain, that, excepting

the observations of the magnetic inclination, made at anchor or on shore, (and even these, as any one may perceive, were far from being certain,) no manner of useful certitude or judgment could arise from them. Mr. Wales, in his description of the dipping needle here spoken of, and made by Mr. Nairne agreeable to a plan of the Rev. Mr. Mitchell, F. R. S. observes, in page 50 of the Introduction aforesaid :

“ The principal defects in this construction are, the difficulty in placing the wires which carry the two last mentioned balls in the proper plane, and the total impossibility of knowing when they are so: moreover, it is very possible, and undoubtedly often happens, that the axis of the needle and its two poles do not lie in the same plane, in which case another difficulty will arise in adjusting the needle to great exactness.” And in page 15 of the Observations themselves, Mr. Wales remarks as follows :

“ The dipping needle which we took ashore at this place (the Cape of Good Hope) was so much out of balance, and so difficult to get in again, that notwithstanding we both of us (Mr. Wales and Mr. Bayley) spent all the leisure time we had from other observations, we did not get it perfectly adjusted before we went away, and of course were not able to get any observations of this kind at this time.”

Mr. Bayley, in page 217 of the Original Observations made in the course of a voyage to the Northern Pacific Ocean, for the discovery of a north-east or north-west passage, in his Majesty's ships the *Resolution* and *Discovery*, in the years 1776, 1777, 1778, 1779, and 1780, after, as appears, much precaution on observing with, and changing the poles of the dipping needle, &c. says: On the 28th July is mentioned an accident happening to the dipping needle: “ the poles were not then changed, as the observations afterwards appeared regular; neither were the poles changed on receiving it on board, as it was said to be well balanced: but the not doing it at either of these times was a great oversight, as we do not know the error of the dips with the marked end north, and dipping in any observations before to-day, and therefore cannot correct them according to Mr. Cavendish's directions. If we suppose the same error from the first as on to-day, (the mean

mean dip with the marked end north, exceeding the mean dip with the marked end south by  $8^{\circ} 8'$ ), then are the mean dips from the beginning to be diminished by  $\frac{488}{2}$  or  $244 \times$  cosine of the dips. If, again, we suppose no error on receiving it, nor from the above accident, the mean dips to this day will be lessened by  $\frac{244}{2}$  or  $122 \times$  cosine of the dip."

What error or difference arose between the extreme of the observed dips with the marked end north, and those with the marked end south, are no where set down; they must, however, undoubtedly have been greater, and in all likelihood much greater than  $8^{\circ} 8'$ , seeing the mean result of the whole, small and great, gave  $8^{\circ} 8'$ . The observations made by Lord Mulgrave are of the like nature and uncertainty.

The whole of these instruments were therefore totally inadequate to the correct and useful purposes of navigation, or indeed to any correctly useful purpose whatever: they were however, as before observed, the very best that could be obtained; and in the making of which no expence whatever on the part of the Board of Longitude, nor pains on the part of those who made them, were wanting to render them as perfect as possible, and such as should answer the purposes for which they were designed; nor do I entertain any doubt they were as much so as the nature of their construction could possibly admit of.

My pursuits in life having been considerably connected with objects of this kind, my own experience, corroborated by the above and like testimonies, together with the continual necessity, which arose from time to time, of increasing the magnetic orbit so as to correspond to the increase of the variation (on the supposition that the true dip and variation might be accounted for and predicted,) by means of moveable magnetic poles \*, led me into endeavours to investigate

\* William Whiston, M. A. in the 8th page of an historical preface to a pamphlet of his, entitled, "The Longitude and Latitude found by the Inclinator or Dipping Needle," gives us the brief history of all theories then published, in these words, speaking of the magnetic poles and theories:

"However, this notion in general of moveable magnetic poles has always

tigate some theory more conformable to the laws of Nature\*, and which would reconcile the continual disagreement between men of science on this subject: and this desire also of necessity led me into endeavours to form more perfect nautical instruments, for without more accurate observations no essential utility whatever could arise to navigation from any kind of theory however perfect. To detail the train of reason-

ways seemed so probable, or rather necessary, ever since the variation has been itself found to vary, and this after a certain regular manner; also, that Mr. Phillips before Mr. Bond asserted the same, and stated the revolution to be in 370 years; and after all, our learned Dr. Halley, who has far outdone every body upon this subject, has determined it to be so, only he has thought himself obliged to add the hypothesis of two other fixed poles; and from the joint effects of all four poles, and from those only, has he been able to bring this variation of the variation to some kind of system agreeable to the observations. He has also been obliged to lengthen the period of the moveable poles' revolution; and as Mr. Bond had enlarged Mr. Phillips's number from 370 years to 600, so has Dr. Halley enlarged the same farther, from 600 to 700 years;" and Mr. Whiston, in page 58 of the pamphlet itself, states the revolution of Dr. Halley's inner nucleus, or north magnetic pole, to be in 1920 years, which, he observes, is much slower than that of Mr. Phillips, Mr. Bond, or Dr. Halley.

Since Whiston's time, those who attempted the magnetic theories are: the celebrated Euler, who, as all others have hitherto done, supposes that the magnetic poles move; Mr. Lorimer, Mr. Churchman, Mr. Walker, &c.; but of all these, Mr. Churchman is the only person that determines their revolution, which he states to be, the northernmost in 1096 years, and the southernmost in 2289 years.

I shall forward, for a future number of the Philosophical Magazine, a more full account of the theories that have hitherto prevailed, in which I shall enter more fully, than my present limits will allow, into the truth of my new theory, and of the method by which I have been enabled to prove the fallacy of the former. In the mean time any person, even by roughly perusing the observation of latitudes, longitudes, and dips, made on the north-west coast of America, and comparing them with that at London, and also with those made off the east coast of South America and in the Chinese seas, will soon be enabled to ascertain, sufficiently near for the purpose of remaining satisfied, that the situation of the magnetic poles is as described: my determinations are, however, drawn from a multitude of calculations, and the result compared with all the magnetic dips and variations (taken in all the different places on shore) I could procure.

\* I account for the change of the magnetic variation by *original magnetic meridians* and a *magnetic annulus*. N.



ing and consequent investigation I went through, would be unnecessary here. The result, which I have long since explained to many individuals, I now, in as brief a manner as the subject will admit of, lay before the public, not doubting that an object, which actually tends to the enlargement and perfection of science, and, in my opinion, to the benefit of navigation, will find favourable reception, investigation, and experiment.

*Method of discovering the Longitude, the Magnetic Inclination and Latitude of the Ship being given :*

PROBLEM the First, being the THEORY.

*To determine the Latitude and Longitude of the Magnetic Poles, and, in consequence, for the purpose of discovering the Longitude, to ascertain a First Meridian.*

Let N (Plate VIII.) represent the north pole of the earth, M the magnetic north pole, S Smeerenberg harbour in Spitzbergen, and L London: also, let arches of great circles be drawn from London to the north magnetic pole, to the north pole of the earth, and to Smeerenberg, and also from Smeerenberg to the north magnetic pole. There will thus become formed three spherical triangles, which are resolved as follows:—

1st, In the triangle NLS are given the sides NL and NS, being the co-latitudes of London and Smeerenberg, and the included angle LNS being their difference of longitude, to determine the angle of position NLS and the third side LS.

2d, In the triangle LSM are given the three sides LS, (just found) LM and SM (LM and SM being the magnetic polar distances corresponding to the correct dips at London and Smeerenberg), to determine the angle MLS, from which subtracting the angle NLS lately found, there remains the angle of position NLM.

Lastly, In the triangle MLN are now given the sides LM and LN, and the included angle MLN, to find the angle LNM, the longitude of the north magnetic pole (and the meridian of the earth passing through the magnetic poles  
and

and poles of the earth,) from the meridian of London, and the side MN the distance of the magnetic pole from the pole of the earth.

The place of the magnetic poles being thus obtained, the question next divides itself into two points of view, namely, Whether the magnetic poles or points thus determined are stationary or otherwise? the solution of which depends on observation and philosophic inference. From observation it is remarked, in page 121 of the late Lord Mulgrave's Account of his Voyage towards the North Pole, that—

“ There is no reason at present to suppose that the dip is liable to any variation in the same place at different periods of time. It having been observed at London by Norman, who first discovered it in 1592, to be  $71^{\circ}.50'$ , and by Mr. Nairne, in 1772, about  $72^{\circ}$ : the difference between these observations, taken at such distant periods of time, is smaller than that found between several of Mr. Nairne's observations compared with each other; and therefore we have no reason to conclude that the dip has altered since Norman's time. The care with which his instrument was constructed, and his observations made, leave us no room to doubt of their accuracy.”

Again, Mr. Cavallo, in his Treatise on Magnetism, p. 65, remarks, (speaking of the dip,) “ Its alteration in the same place at different times is very small; thus in London, about the year 1576, the north pole of the dipping needle stood  $71.50$  below the horizon, and in the year 1775 it stood at  $72.03$ : the alteration of the inclination in so many years amounting to less than a quarter of a degree, which may be attributed to the error of the instruments, since, as will appear in the sequel, those instruments are far from having attained to any degree of perfection even in the present age.”

Now, from philosophic inference it will naturally follow, that if the inclination of the magnetic needle at different periods of time at any one place remain constant, the magnetic poles are stationary; and if the magnetic poles are stationary, (as appears clearly evident from above,) the meridian thus discovered is the *First* (or only) *Meridian* from which the longitude

gitude ought to be counted; I have always used it as such, and obtained the most satisfactory results.

PROBLEM the Second, being the PRACTICE.

*To determine the Longitude universally.*

Here are always given the ship's polar distance, the magnetic polar distance, and the magnetic co-latitude, being three sides of a spherical triangle, to determine the angle at the pole of the earth opposite the magnetic co-latitude, being at all times and places the longitude of the ship east or west (as the case may be) from the meridian aforesaid.

N. B. The magnetic co-latitude is the distance of the ship from the nearest magnetic pole; the ship's polar distance is the distance of the ship from the pole of the earth next adjoining the said magnetic pole; the polar distance is the distance between either magnetic pole and the pole of the earth nearest thereto: moreover, the longitude thus obtained can never exceed 180. It may, however, be easily reduced as to that estimated from any assumed first meridian. Likewise the point M, thus assumed as the magnetic pole, may, to prevent perplexity, be better understood as being that point on the surface of the globe at which the dipping needle stands at right angles to the plane of the horizon; without regarding whether the magnetic poles themselves are under, at, or above the surface of the earth.

The method of obtaining and establishing (from observation alone) certain data for investigating or corroborating perfect tables of magnetic latitude and co-latitude, appears (from the preceding theory) obvious, being, by carefully making different dips of the magnetic needle along the meridian aforesaid, and carefully observing the respective latitudes at which those dips are taken, and for greater certainty (if thought needful) continuing them, as far as possible, into both hemispheres; though it will also be perceived they may be investigated by observing different dips, along any other great circle, passing through the magnetic poles, or having determined the precise latitude and longitude of the magnetic poles. Correct tables may also be deduced from correct dips at various places; however, the

first method is not only the most easy, but on many accounts the most preferable; and for this purpose the correct dips of the magnetic needle, taken at the following places, (regard also being had to determine their correct latitudes and longitudes, particularly the former, except near the magnetic poles, where it is necessary to correctly determine both,) will be found sufficient, namely, at the Falkland Islands, Buenos Ayres, Oronoque, Trinidad, Barbadoes, Saint Luke, Martinique, Guadaloupe, Antigua, Saint Martin's, Bermudas, the Atlantic Ocean, in or near the same longitude up to and near the Isle of Sables, at the Island of Cape Breton, the south side of the river Saint Lawrence in the longitude aforesaid; then going round to the north-east shore of the Labrador Coast, and observing thereon at Davis's Inlet, and so on up to the northward under the meridian aforesaid, up to and into Baffin's Bay, and there carefully determining the latitude and longitude of the north magnetic pole; or first going into Baffin's Bay, and afterwards proceeding to the southward.

Next, by carefully observing the magnetic inclination along the opposite meridian in Russia, China, the Chinese Seas, Indian Ocean, &c.; as at the mouth of the Leno, at Olikminkoi, at Nutschink or Albazin, at Pekin, Nankin, Nimpo, Formosa, the Philippine Isles, Gindano, Celebes, Flores, the north and south side of New Holland, and so continuing to observe along the Indian Ocean, in or near the meridian aforesaid, up towards the south pole of the earth, until the place of the south magnetic pole be carefully determined, *i. e.* whatever pole may be first convenient to determine. The number of places herein set forth are mentioned in order that navigators and others, as opportunity may offer, may know where and how to make the advantageous observations herein alluded to.

It may also be proper to add, that hitherto the places of the magnetic poles have always been attempted to be found by means of the magnetic variation; a method whereby the most able mathematicians have been continually led astray, for, as the magnetic variation is continually changing, their  
conclusions

conclusions were rendered as uncertain as the variation itself, of which (it had so happened from the method they pursued) they knew little more than that it did change. Had the present theory occurred to them, it would have received cheerful and favourable countenance and encouragement, and it and its utility been soon and effectually established. Had the indefatigable and famous Dr. Halley in particular known thereof when he undertook two voyages (long after the inclination of the magnetic needle was discovered, namely, in 1698 and 1699) for the express purpose of endeavouring to form a magnetic theory by means of a multitude of observations of the magnetic variation, as a preparatory step, and in order to determine the longitude thereby, or, as his instructions run, “to seek, by observation, the rule of the variation of the compass;” the observations of magnetic dip here recommended, and which he would have made at least along the western line before pointed out, together with the observations of the variation of the compass, which he did observe, in all likelihood, if made with instruments capable of affording the requisite accuracy, would have soon suggested, to his penetrating and fruitful genius, the correct object of his research.

It may be farther proper to remark, that the observations of the magnetic inclination herein stated, and made at London, do of themselves alone establish another important conclusion, likewise quite different from the general opinion of the Learned, which supposes that the magnetic variation affects (*i. e.* increases or diminishes) the magnetic inclination; for, from the time in which the magnetic inclination was first observed at London to the present time, the variation has shifted upwards of  $36^{\circ}$  without affecting the inclination; and therefore, after knowing the result of so very great a change in the magnetic variation without having affected the inclination, there can be no reason to conclude that it can in any respect be affected thereby: a circumstance that renders the theory here pointed out still more simple and beautiful.

Thus, by new and interesting application and testimony of unerring science, practical observation, and fair conclusion,

I have, I flatter myself, cleared the magnetic theory of the abstruse and embarrassing considerations which have hitherto bewildered it, and thereby laid the foundation of a most simple, general, and useful method of determining a ship's correct place at sea, without any regard to the sun, moon, or stars, to good or bad weather; to the time of the day, the day itself, months, years, seasons, or centuries, except so far as may be said to regard the magnetic inclination, which must always be known from observation; as also the latitude of the ship, which must likewise be known, either from observation, or dead reckoning.

I would not have it understood, however, that I mean to decry the use of lunar observations or chronometers. Far otherwise: I have devoted great application to the study of the lunar (and other) astronomy; and herein I am happy to add, I have succeeded to an accuracy and conciseness of equations and calculations far surpassing Mr. Mayer's, or any lunar tables yet offered to the public. But herein, notwithstanding the high esteem and veneration in which the great genius and labours of Euler, and the merits of Mr. Mayer must ever be held by men of science, I found it necessary to reject, for reasons which I shall hereafter transmit for insertion in the Philosophical Magazine, the supposition that the motion of the sun or earth, according to Mr. Euler, and of the moon according to Mr. Mayer, are different now to what they formerly were. I am also the original inventor, and for which I have, for upwards of seven years, had a patent, of a double sextant for making lunar observations to the right and left. I have also invented a metal quadrant, by which, with fore-adjustments and observation, the distance of the moon can be measured from the sun or other object, from one limit or extreme point of the horizon to the other.

From the multitude of lunar places which I have calculated from my tables, I am persuaded they are such as shall not, even at the equator, produce a mean error of five, nor an extreme error of fifteen miles in longitude. The first of the instruments here spoken of (a double sextant) measures with convenience, without inverting the instrument, the simple limits of a sextant on either side the sun or a fixed star:

to the other there can be no limits, for it measures 180 degrees on either side. Not satisfied with doing thus much, in order to avoid the inconvenience, trouble, and inaccuracy arising from the use of large volumes of charts of spherical triangles, and of tables for reducing observed lunar distances to true, I contrived an instrument, for which, as well as several superior surveying and other instruments, I have also got a patent. This instrument, which I have denominated a suit of circles of calculation, performs this operation with ease, accuracy, and expedition. It also determines the true latitude, without knowing the latitude by account, either by double altitudes and the time between, by double altitudes and the difference of magnetic azimuths, (which my instruments will correctly give,) or by simply having the altitude of two known fixed stars. It determines the time of the day, &c. &c. In a word, it resolves all manner of spherical triangles or spherical trapezia, &c. The resolution of the few of these that constitute the essentials of nautical astronomy and geography, becomes by this means obvious, easy, and pleasant; whereas the understanding, or even the appearance of the previous burthen of requisites, creates, as is well known, a great if not insuperable obstacle to the generality of otherwise well-informed seamen.

Having thus, I trust, explained my regard and esteem for the lunar observations and chronometers, all I would have understood of the method of discovering the longitude by the dip is, that it is easily understood by the most ordinary capacity; that to others, in every instance, it becomes an auxiliary or corroborating proof, and, in the time of greatest necessity and peril, a most estimable substitute; not requiring, in this case, any calculation whatever. Thus, from the preceding theory, the dip in all places remaining the same, the navigator, being perhaps several days without seeing sun, moon, or stars, so as to make any use of either towards determining his longitude, has this benefit left him, that he can, at any moment of the day, in which the extremity of the horizon can be seen, determine his correct dip, and therefore his vicinity towards the land he wishes to make or avoid; where otherwise he might either be cast away, or, under  
the

the supposition that he was near thereto, keep either lying-to, or straining his ship in endeavouring to get no farther to leeward, when at the same time he might be an hundred miles or more from shore, and so lose the very wind he wanted; after which, should contrary winds assail him, the injury also becomes obvious.

*To clear apparent or observed Lunar Distances from the Effects of Refraction and Parallax by the Suit of the Circles.*

Set the vernier on the first meridian, carrying the circle or semicircle of position to the apparent altitude of the sun's centre, or that of the star; the vernier on the moveable meridian to the apparent altitude of the moon's centre; and the vernier on the circle or semicircle of position to the apparent distance of the sun and moon's centres, or of the centre of the moon and fixed star. Let these form a spherical triangle; so shall the number of degrees, minutes, and seconds on the equator, comprehended between the first and moveable meridian, shew the angle (or difference of azimuths) at the vertex, which angle is common to the apparent and true triangle. Keeping fast, therefore, the said meridians by means of the equator, set the vernier on the first meridian to the true altitude of the sun or star's centre, and the vernier on the moveable meridian to the true altitude of the moon's centre; then see what number of degrees, minutes, and seconds of the circle or semicircle of position or distance are contained between them, for that is the true distance.

*Portsmouth, Dec. 13, 1795.*

WE certify, that, in our opinion, a compass and sextant invented by Mr. Nugent merit an expeditious and accurate investigation and trial.

(Signed)

T. PACKENHAM, *Le Juste.*

J. CRAUNSTON, *Bellerophon.*

E. GOWER, *Triumph.*

W. DOMETT, *Royal George.*

R. BOWEN, *Terpsichore.*

The foregoing certificate was given to me, unsolicited, for the purpose of being transmitted to the Lords Commissioners of



of the Admiralty, in order to procure their Lordships order to have the merits and efficacy of my inventions and instruments officially examined into and reported upon. As to the instruments, they were on the same day applied for, through Commissioner Marsh, to the Navy Office, by the above gentlemen.

The most speedy and least expensive method of ascertaining the precise situation of the north magnetic pole, the southern being opposite thereto, appears to be, by engaging one of the Davis's Strait whalers to proceed into Baffin's Bay, with a proper person duly qualified, to make the necessary astronomical and magnetic observations; or rather, by dispatching for this purpose a frigate or other vessel in the king's service employed on the Newfoundland station. Pity it was, indeed, that the Board of Longitude had not known of this method, and of the reasons that render all navigation into the South Sea, by any passage to the eastward of Greenland, totally impracticable. The Hon. Captain Phipps's destination would in that case have been to proceed into Baffin's Bay, where, had he also possessed the angular and magnetic instruments herein alluded to, he would have assuredly ascertained one great object of his voyage, namely, the determining the fallacy or utility of all previous theories, and bringing them to the test of experiment; a test which would have caused the whole to have vanished, and none other would have remained but that I have already pointed out. Nor do I entertain much doubt that, by attending to circumstances, which might easily be pointed out, Captain Phipps would have also accomplished a passage into the South Sea.

The principles of the whole of my nautical magnetic instruments (except a variation frame for converting all steering into variation compasses) is the application of a quadrant, sextant, or double sextant, (fitted for this purpose,) to the vertical magnetic axis of the horizontal needle for determining the variation, and to the lateral magnetic inclinatory axis of the dipping needle for determining the dip. These needles are also differently suspended from others: the dipping needle, for example, is always at liberty to resort to, and remain  
at

at rest in, the common intersection of the lines of the magnetic meridian or variation and dip; nor can any alteration of direction or motion of the ship affect a dipping needle thus suspended.

In consequence of this simple contrivance, the motion or gyration of the graduated circles for either dip or variation on account of the motion of the ship, becomes totally avoided; for, on or in these magnetic instruments there are no divisions whatever: the instruments themselves can be afforded for half the price of others; the navigator makes his observations like a man of science; and finally, he determines with expedition, ease, and certainty, the magnetic dip and variation, to degrees, minutes, and seconds, *i. e.* to the like accuracy that he can distinguish or read the subdivisions shewn by the vernier on the arch of his sextant: a circumstance utterly impossible to be obtained even to whole degrees, it might be said to half a dozen degrees, by any other constructed instruments, as has been herein before sufficiently verified.

XII. *Description of a Portable Machine for loading and unloading Goods.* By Mr. GEORGE DAVIS, of Windsor, Berks\*.

THE ingenious contriver of this machine is certain, that, when made of its intended size, it will be capable of loading a ton weight by one man only, and will be so portable as not to exceed one hundred and twelve pounds in weight.

*Reference to the Figure of the Machine.* (See Plate IX.)

A, the winch, which turns the bar B. This bar has on it two endless screws or worms C, C, which work in the toothed wheels D, D. These wheels are fixed to the barrels E, E, round which the ropes F, F, coil, wind up, or let out the same occasionally; which ropes, passing over the two

\* From the *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce.*

A premium of forty guineas was given to Mr. Davis for this invention.

pulleys G, G, are brought round, and their ends, having hooks for that purpose, are hitched into staples fixed to the front of the cart or other carriage: within these ropes the load H is placed on a common pulley I, which forms an inclined plane, up which, by the turning of the winch, the ropes are wound upon the barrels, and the load raised into the carriage.

K K, the frame, intended to represent the part of the cart, or other carriage, on which the machine is occasionally to be placed.

The whole of the barrels and cogged wheels are contained in an iron box L, the sides of which are represented in the figure as taken off, that the construction of the several parts may be shewn.

XIII. *Description of a cheap and efficacious Ventilator for preserving Corn on Ship-board.* By THOMAS SOUTH, Esq. \*

THE importation of grain is a precarious traffic. The produce of distant countries, or even of those near home, when long in collecting, or long detained on ship-board, is subject to heat, soon becomes fetid, and is often so far spoiled and depreciated in its value as to sell for less than the original cost. Hence the merchant, overwhelmed with losses, regrets his patriotism, grows shy of importation, and, unless invited by a certainty of gain, drops the trade, even whilst the nation stands in need of supplies.

The remedy here proposed is a simple, cheap, and, I trust, efficacious method of ventilating grain whilst confined on ship-board; sufficient, I presume, to keep it sweet and marketable after sustaining a tedious voyage.

*Description of the Ventilator, with References to the Figures thereof.* (See Plate VII.)

Fig. 1. is a cylindrical air-vessel or forcing-pump, of lead,

\* From the *Letters and Papers of the Bath and West-of-England Society for the Encouragement of Agriculture, &c.*

tin, or other cheap metal; its internal diameter being ten inches, and its length three feet; having a crutch-handled piston to work with, and an iron nosle, *viz.* a hollow inverted cone, two feet long, to condense the air, and increase its power in its passage downwards. This cylinder should be riveted or screwed, by means of an iron collar or straps, to the deck it passes through, both above and below, as at *aa*; and should be farther secured by some hold-fast near *b*, to keep it steady in working.

Fig. 2. is a bottom of wood, four inches and a half thick, with a projecting rim at its base, for the metal cylinder to rest on, when cemented and screwed to the wood. The centre of this bottom is excavated, for the reception of the crown of the nosle. In the same figure the nosle is represented with its crown like a bowl-dish, to condense the air gradually, without resistance, in its advance to the more contracted base of the inverted cone, *i. e.* the top or entrance of the nosle. About two-thirds down this nosle may be fixed a male screw, as *cc*, for the purpose hereafter mentioned.

N. B. The forcing-pump should be cased in wood, to protect it from outward bruises, which would prevent the working of the piston, and ruin its effects. The leather round the embolus should be greased when used.

Fig. 3. is a crutch-handle, fastened to the embolus *A* by its iron legs *B, B*. *A* is a cylinder of wood, cased with leather, so as to sit well, but glide smoothly in the metal cylinder; having an opening as large as its strength will permit, for the free access of atmospheric air. *C* is a valve, well leathered on its top, and yielding downwards to the pressure of the air when the piston is raised up. *D* is a cross bar of iron, to confine the valve, so that it may close instantly on the return of the piston downwards.

Fig. 4. is a tin pipe or tube, of less than four inches diameter, and of such length as, when fixed to the base of the cylinder, Fig. 1, shall admit the nosle *J*, Fig. 2, to within half an inch of the valve *E*, at the bottom of the wooden cylinder *F*, in Fig. 4; which valve *E* will then yield to the pressure of air condensed in its passage through the nosle, and deliver it into the pipes below. This valve must be well  
leathered

leathered on its upper surface, and fastened with an hinge of leather to the cylinder it is meant to close: affixed to its bottom is the spindle *G*, passing through a spiral spring *H*, which, being compressed on the descent of the valve, will, by its elasticity, cause it to rise again, close the aperture above, and retain the air delivered beneath it. On connecting this cylinder with the upper end of the nose, at *ee*, Fig. 2, we must carefully prevent any lapse of air that way, by a bandage of oakum smeared with wax, on which to screw the cylinder, like the joints of a flute, air-tight. *I* is a bar of iron, having a rising in its centre, wide enough for the spindle to play through, but at the same time sufficiently contracted to prevent the passage of the spiral spring.

Fig. 5. is an assemblage of tin pipes, of any lengths, shaped suitably and conveniently to their situation in the ship, to the form of which, when shut into one another, they must be adapted; observing only, that the neck be straight for a length sufficient to admit the lower end of the cylinder, Fig. 4. as high as the letter *F*, or higher.

Fig. 6. To the middle pipe, which runs along the bottom, should be fixed a perpendicular one, fully perforated, to convey the air more readily into the centre of the heap; and this may have a conical top, as represented in the plate, perforated with a smaller punch to prevent the air from escaping too hastily. In large cargoes, two or three of these perpendiculars may be necessary; and each should be well secured by an iron bar *g*, screwed down, to prevent their being injured by the shifting of the cargo in stormy weather or a rolling sea. The top of the conical cap of these pipes may reach two-thirds up the cargo.

Fig. 7. is a valve of the same construction as that represented in Fig. 4, but inclosed in a tube of brass, having a female screw at *ff*, adapted to the male screw *cc*, on the nose Fig. 2, and may then be inserted into the head of the pipe Fig. 5. This will add to the expence; but, in a large apparatus, is to be preferred, as a more certain security from lapse of air, than the junction of the tube Fig. 4, to the neck *ee* in Fig. 2.

N. B. *ee* is a neck of wood, making a part of the bottom

Fig. 2, whereon to secure the tube Fig. 4, when applied to the nosse. The joints of the pipes, when put together for use, should be made air-tight, by means of bees-wax or some stronger cement, till they reach the bottom of the vessel, when there is no farther need of this precaution. The horizontal pipes should run by the side of the kelson the whole length of the hold. The tin plates of which K is made, should be punched in holes, like the rose of a watering-pot, in two or three lines only at most, and then formed into a tube, with the rough side outwards. L may have four or five lines of the like perforations. M, and the rest, should gradually increase in their number as they advance towards the middle of the hold, and continue fully perforated to the last pipe, which should be closed at its end to prevent the ingress of the corn. It is the centre of the cargo which most requires ventilating; yet air should pervade the whole. Like the trade-winds, it will direct its course to the part most heated, and, having effected its salutary purpose there, will disperse itself to refresh the mass.

Where the hatches are close-caulked, to prevent the influx of water, vent-holes may be bored in convenient parts of the deck, to be bunged up, and opened occasionally, from whence the state of the corn may be known by the effluvia which ascend when the ventilator is working.

The power of the ventilator is determined by the square of its diameter multiplied into the length of the stroke, and that again by the number of strokes in any given time.

To find the area of a circle, and the solidity of a cylinder raised on that circle, Archimedes gives the following proportion:—

As 1 is to .785398 decimal parts, so is the square of the diameter to the area of the circle.

And, as 1 is to .785398, so is the square of the diameter, multiplied by the height, to the solidity of the cylinder.

The cubical contents, both of cylinders and tubes, are found in the same manner; their difference consisting not in shape, but solidity, the latter being hollow.

Then, to find the contents of a cylindrical vessel whose internal diameter is ten inches, multiply that into itself, and the

the square thus obtained, multiplied by .7854, will give the contents of the circle in cubic inches; which, multiplied again by twenty-four inches or lengths of the stroke, being the proportion of the barrel filled with air, gives in cubic inches the amount of each discharge on the descent of the piston. As thus:

	Inches.
Internal diameter of the pump or tube	10
	× 10
	—
	= 100, or square of the diameter;
which, multiplied by .7854, to bring the contents of the square	
	to the contents of the circle.
Which, multiplied by the } 78,5400 Contents of the area of the circle.	
length of the stroke, }	24 inches, produces 1884 cubic inches.

3141600
1570800
—
18849600

which, divided by 231) 1884.9600 (8.1600 gallons, which is $\frac{1}{108}$ the	
<i>vis.</i> the number of cubic inches —	more than 8 gallons at a
in a wine gallon, quotes 8 galls. 369	stroke; allow these deci-
—	mals for waste of air in
1386	each stroke; and 60 strokes
—	to be made in a minute.

Then - - - - - 8 gallons discharged at a stroke,  
multiplied by 60 the number of strokes

amounts to	480 gallons <i>per</i> minute;
which multiplied by - - 60, the minutes in an hour, produces 28800	
	gallons in that time;

and that, divided by 252) 28800 (114.3 tons.	
(the number of	—
gallons in a	.360
ton, both wine	—
and ship mea-	1080
sure) quotes 114	—
tons in an hour.	.720
	—

Then, suppose the area of the hold of a ship to be = 120 tons, and, when freighted, the interstices between the grains, together with the area between the surface of the corn and  
the

the underfide of the deck = 5 tons = to the quantity of mephitic air confined ; fuch being the lighteft fluid, the major part of it would, foon after the commencement of the operation, be forced, by the atmofpheric air, to vent itfelf at the holes provided for that purpofe ; and the remainder of the hour being employed in the like ventilation, five tons of frefh air would pafs above twenty-times repeatedly amidft the grains, to cool, refrefh, and fweeten the cargo. A purification thus adminiftered once in eight-and-forty hours, would, I conceive, be amply fufficient to preferve the corn from taint or injury, be the voyage ever fo tedious ; and unlefs it fhould by neglect have overheated and grown together, or fettled too clofe, the labour would be that of a boy only ; for the dairy-girl at her churn works harder than he otherwife need to do at this.

My air-veffel is, for the fake of cheapnefs, confined to the narrow diameter of ten inches ; but, as the contents of circles are proportionate to the fquare of their diameters, by enlarging that, you increafe their power accordingly ; wherefore, by extending the diameter to fourteen inches, the contents will be nearly doubled ; and, by adding ten inches more to the length of the ftroke, you almoft treble the difcharge of No. 1, and obtain a power capable of ventilating a cargo of 400 tons within the hour. But the air-veffel muft be lengthened ; the pipes at the fame time enlarged ; the metal of which the whole is conftituted be in fubftance proportionable ; and the labour be that of a man, or perhaps two upon occafion.

A ventilator, on the plan and dimenfions here propofed, would come within the compafs, I fhould think, of five or fix guineas. One on the larger fcale, caufed by the increafed fubftance of the metal, and the extra fize and length of the pipes, might amount to twenty ; which, in either, is under fourpence *per* quarter on the firft cargo ; and as they will laft many years if well painted, and, when not in ufe, taken to pieces and put carefully by, I flatter myfelf it is an experiment well worth trial ; particularly if a premium be offered to the fhip-owner, who, by means of fuch machine, imports his corn pure and untainted from a diftant land.

*Objections*



*Objections made to the supposed Effect of the Ventilator, over-ruled, it is hoped, by the Considerations which follow them.*

First, The holes pierced in the tin tubes which are to lie under the corn, seem capable of issuing (especially if an effort be made upon them) a much larger quantity of air than the forcing-pump will supply in a given time. Consequently, a given quantity of these holes, under a given pressure, will be capable of issuing the whole supply of air, without any assistance from the remainder.

Secondly, If these positions are just, it must happen, that if a cargo of corn be unequally circumstanced in relation to its permeability, the whole of the air discharged by the pump will issue through the *more* permeable parts of it, without affecting, in any degree, the *less* permeable ones.

Thirdly, In cargoes heated in any degree, and in those infected by that worm which fastens grains together by a web, the parts most affected become much more close and densely packed together than the rest, either by the swelling of the heated grains, or by the web and dung of the worms which occupy the intervals between the grains.

If so, the parts of a cargo which require the most ventilation will receive the least; but, in all cases, it seems likely that the air discharged will not regularly permeate the whole of the cargo, but will pass through the parts where the grain lies lightest, and leave untouched those parts where it is most closely packed together.

*Answer to the preceding Objections.*

Though the holes appear numerous, they must be small, lest the corn gain admission; and many (especially of the uppermost) will be nearly, if not totally, stopped by the pressure of the grains upon them. Besides, the pipes which convey the air towards the centre are not meant to be so fully perforated as those at and beyond it; and all may be still less so, if in practice found necessary. But as the quantity of air delivered by the forcing-pump within five seconds

of time is equal to the contents of sixty \* feet of four-inch pipe within the first minute, the air (notwithstanding the manifold perforations, obstructed as it is in meandering through a mass so nearly compacted as the bottom of the cargo must necessarily be by the pressure of the heap above) will undoubtedly reach to the end of the pipes, and consequently affect the cargo even there.

Be it farther observed, that the flux of air compressed into an half-inch stream, in its passage through the nozzle, to enable it to overcome the resistance of the spiral spring H, no sooner passes the valve E, than it expands itself to the compass of the pipe; by which expansion, and extension (at the

\* Thus calculated :

	Inches.
60 feet	4
× 12 inches.	× 4
<hr/>	
Produces 720 inches	16 the square of the diameter of the
as a multiplier.	× 7854 pipe.
<hr/>	
= 12,5664 or area of the circle.	
× 720, length of the pipe in inches.	
<hr/>	

Which, divided by 231)9047,8080(39,1679 gallons and decimal parts,  
the whole capacity  
of 60 feet of pipe.

$$\begin{array}{r}
 2117 \\
 \hline
 ..388 \\
 \hline
 1570 \\
 \hline
 1848 \\
 \hline
 221, \&c. \\
 \hline
 \end{array}$$

Then, a single discharge of the forcing-pump being eight gallons, five such discharges amount to 40 gallons, which is more than equal to the contents of 6 feet of four-inch pipe.

And as on the larger scale of ventilators the pipes need not exceed the same diameter, the power of the air injected, when its egress is stopt, will increase sufficiently to force its way through webs, mats, and other obstructions, though impervious to the atmospheric fluid, unassisted by such mechanic aid.

same

same time) forwards, its power becomes so weakened, that small egress only will be made, till the pipes are filled with a fluid more dense than atmospheric air, which will then, as is justly noticed, issue where it finds the least obstruction, unless attracted to the spot most heated.

Many circumstances may cause one part of the cargo to be less permeable than the rest; should it prove so, the means readily offer for airing and purifying even this.

Suppose the hatches to be caulked down, and the hold made impervious to water; in such case, the lapse of air, under the obstructions met with in its passage, could by no means keep pace with the influx from the forcing-pump; consequently, if the holes in the deck, designed for its exit, be kept close-stopped till the pumper feels resistance, all the intervals of the cargo, be they ever so minute or irregular, must be occupied by fresh air, which, when permitted to escape, will carry off impurities with it. And thus, by stopping and opening such vents repeatedly, no part of the cargo could miss of purification. and this perhaps may be the best mode of administering it.

Prevention is better than a cure.

In a vessel equipped with the apparatus described, the inattention must be great, if the corn be suffered to sustain any injury at all. By an early use of it, perspiration and damp will presently be dried away; heating of course will be prevented; and even the production of the pernicious grub alluded to: for, be the nidus of its eggs ever so productive, their embryos will not vivify, without moisture to sustain them. Wherefore, it should seem that the corn-merchant in future will have little to fear, save the influx of sea-water; and even this (if in small quantities) will, by the frequent use of the ventilator, gradually dry away.

## INTELLIGENCE,

AND

## MISCELLANEOUS ARTICLES.

## ROYAL SOCIETY OF LONDON.

ON the 30th of November last, the anniversary meeting of the Society was held. Sir Godfrey Copley's gold medal, which is disposed of yearly on this day to the most deserving member, was awarded to the Rev. Mr. Hellings.

The receipts of the Society for the year were declared to have been 1736 *l.* and the disbursements 1563 *l.*

The meetings of December the 5th and 12th were occupied principally in reading the conclusion of Dr. Herschel's paper on the power of penetrating into space by means of telescopes. According to the Doctor's observations, it is impossible with the naked eye to see any star smaller than those of the 8th, 9th, or 10th magnitude. With his 40 feet reflector he has discovered stars which must be three hundred thousand times more distant than the nearest fixed star. Where are the bounds of creation!

At the latter meeting, an abstract of a register of the weather at Lyndon in Rutlandshire, kept by Mr. Barker, was read; as was also a paper on annuities, respecting the contingencies of three lives, by Mr. Morgan.

December 19th. A paper, by Mr. Jordan, on the irides surrounding the sun, moon, &c. was partly gone through, when the Society adjourned for the Christmas holidays.

January 9th, the conclusion of Mr. Jordan's paper was read; as was also a paper by Mr. Anthony Carlisle, on some peculiarities in the arterial system of certain animals.

On the 16th and 23d, a paper on sound.

## COMET.

C. Lalande has announced that, on the 26th of December last, his fellow-labourer Mechain discovered a comet near the star  $\epsilon$  of Serpentarius, which could be distinguished by  
the

the naked eye. It had a tail of a degree, and was proceeding in a southern direction.

#### SUBSTITUTE FOR CINCHONA.

C. Zannetini, a physician who attended the French army in Italy, has made some experiments, by which it appears that the flowers and seeds of the common nettle (*Urtica dioica* LINN.) may be employed in fever instead of cinchona. This substitute was attended with a success beyond all expectation, in tertian and quartan malignant fevers. The nettle often produces a speedier effect than bark, for it heats in a great degree, and, when the dose is pretty strong, occasions a lethargic sleep. The dose must never exceed a dram, and is given in wine two or three times in the course of 24 hours. Zannetini found this medicine of great service to guard against that total exhaustion which forms the principal character of malignant fevers; and he recommends a slight infusion of it in wine as an excellent preservative for those who reside in marshy and insalubrious districts. In employing the nettle in fever, Zannetini gives the same caution as ought to be observed in regard to cinchona, that is, that it must not be employed where there is an inclination to inflammation, or where a continued fever, arising from obstructions, exists. This discovery is not unworthy the attention of physicians, and deserves at least to be farther investigated, as a great deal would be saved if cinchona could be entirely dispensed with.

#### CURE FOR DISEASED ELMS.

C. Boucher, secretary to the Society of Emulation at Abbeville, has lately published a memoir on the diseases which attack elms, and the method of curing them, from which the following is an extract:—"Elms are frequently attacked by ulcers, which at length destroy a great number of these valuable trees. Duhamel supposed that this malady might be ascribed to a plethora of the sap; and C. Boucher, by numerous experiments, has established this fact, and discovered a remedy. He observed, that local ulcers never attack the tree on the north side, but almost always on that exposed to the south. The elms chiefly subject to it are those planted in marshy ground, and in the neighbourhood of rivers. The ulcer is generally at a little distance from the earth, seldom

more than five or six feet above it. This disease, arising from a superabundance of sap, differs from another disease of the elm described in the *Journal d'Histoire Naturelle* \* for the year 1789, in this circumstance, that the liquor, when exposed to the atmosphere, soon acquires the consistence of a gum, and has a very saccharine taste.

To cure the trees attacked, C. Boucher pierced each of them with an auger, which he inserted in the ulcer itself, and then fitted to the hole a tube which penetrated to the depth of 1.10 inches. Sound trees pierced in this manner yield no liquor; but those which are diseased give a more abundant quantity, in proportion to the serenity of the weather, and as the wound is more exposed to the south. This effect is suspended by stormy weather and high wind. He observed that at the end of two or three days the efflux of the sap ceased, and that the wound dried up and was healed.

This, therefore, is a simple and easy method of radically curing elms attacked with this disease. It is probable that the same process applied to other vegetables, and particularly to some fruit-trees, would produce a like effect. Pliny, Columella, and Palladius, mention the same means as having been employed by the ancients, but it has not been before practised for many years.

C. Boucher has completed his observations on the elm, and proved that this tree is not an exotic, as some authors have advanced. The study of the ancients has proved to him that it existed in Europe in the remotest periods, and very evident remains of it have been found in old mosses.

The analysis which C. Boucher made of the sap of the elm shewed that it contains a pretty large quantity of the acetite of pot-ash, a little of the acetite of lime, a certain quantity of vegetable matter or saccharine mucus, and a pretty large quantity of the muriat of lime. There exist in it, also, slight traces of the sulphat and muriat of pot-ash. This is nearly the same analysis as that before announced by C. Vauquelin.

#### \* NEW PNEUMATIC APPARATUS.

Mr. H. W. Pepys jun. has constructed a mercurial exhauster on the principle of the Torricellian vacuum. The

inventor has promised to enable us to lay the plan of the apparatus, with some of the most interesting of those experiments which have yet been made with it, before our readers in a future number.

## DEATHS.

On the 19th of December, C. Montucla, author of an excellent history of the mathematics. Two volumes only of the new edition of this work were published before the author's death, but fortunately all his corrections for the remainder were finished.

At Paris, on the 1st of January, the celebrated naturalist Daubenton, the friend and fellow-labourer of Buffon. He was born at Montbard in Burgundy in the month of May 1716. He studied medicine, and intended to follow that profession in his own country; but Buffon, being appointed intendant of the king's garden in 1735, proposed to Daubenton to reside with him, to apply to natural history, and to assist him in the grand labours which he was then about to undertake. In 1740 the fate and taste of Daubenton were determined for his whole life. More than half a century devoted to the formation of the cabinet of natural history, which in 1750 was only a plain drug-shop belonging to Geoffroy, which he arranged methodically and enriched with productions of every kind, has given him a distinguished rank among naturalists. Being admitted into the Academy of Sciences in 1744, he never ceased to enrich the collection of its memoirs with various papers for nearly fifty years. The greater part of them contain new facts and ideas respecting the classification of shells, on the hippopotamus, the shrew-mouse, bats, fossil bones and teeth, the situation of the great occipital foramen in man and animals, rumination and the temperament of sheep, a description of several kinds of new animals, or animals little known. He was interred with great pomp on the 3d of January in the garden of plants. His funeral was attended by more than 500 persons, and two orations were pronounced on the occasion by C. Lacede and C. Fourcroy.

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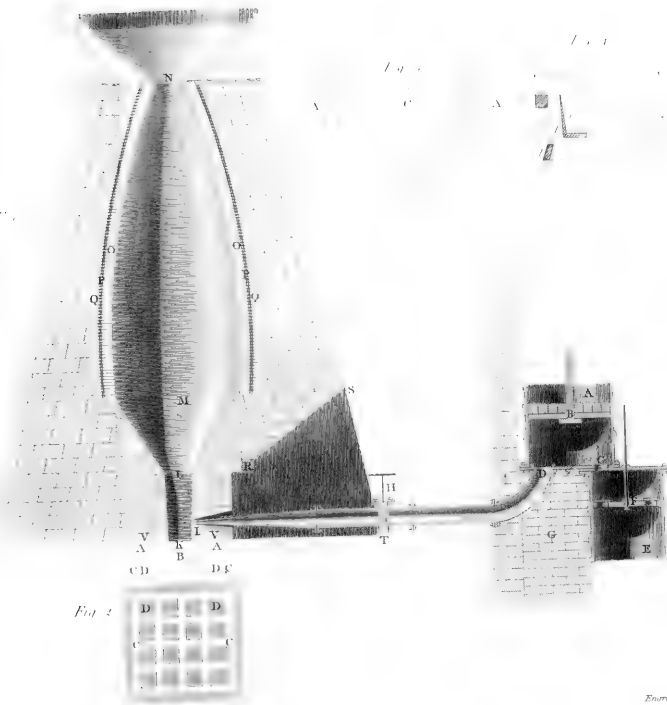


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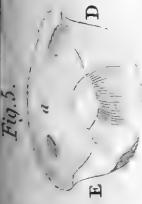


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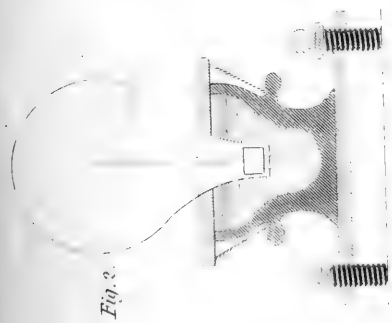
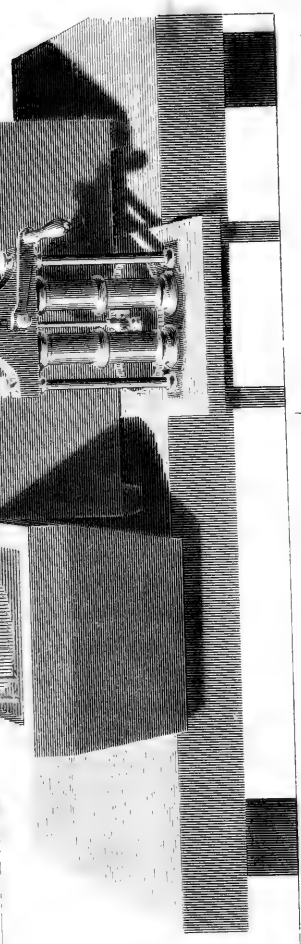
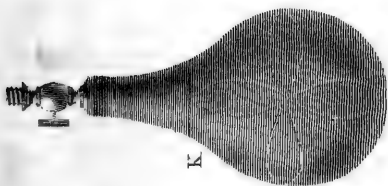
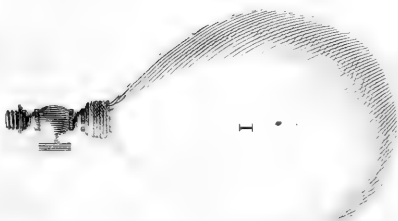
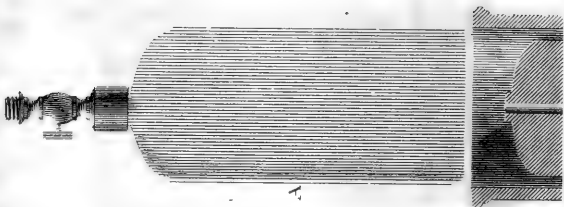


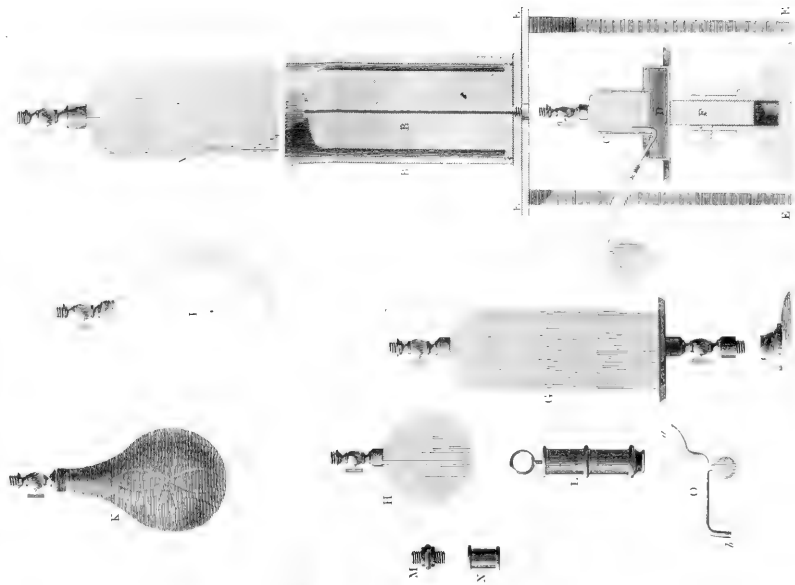
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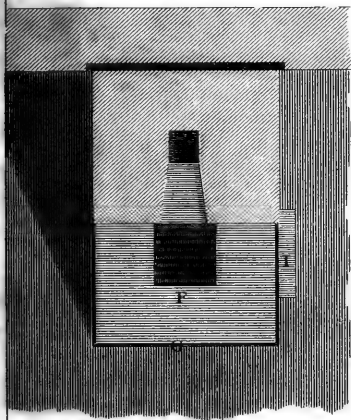




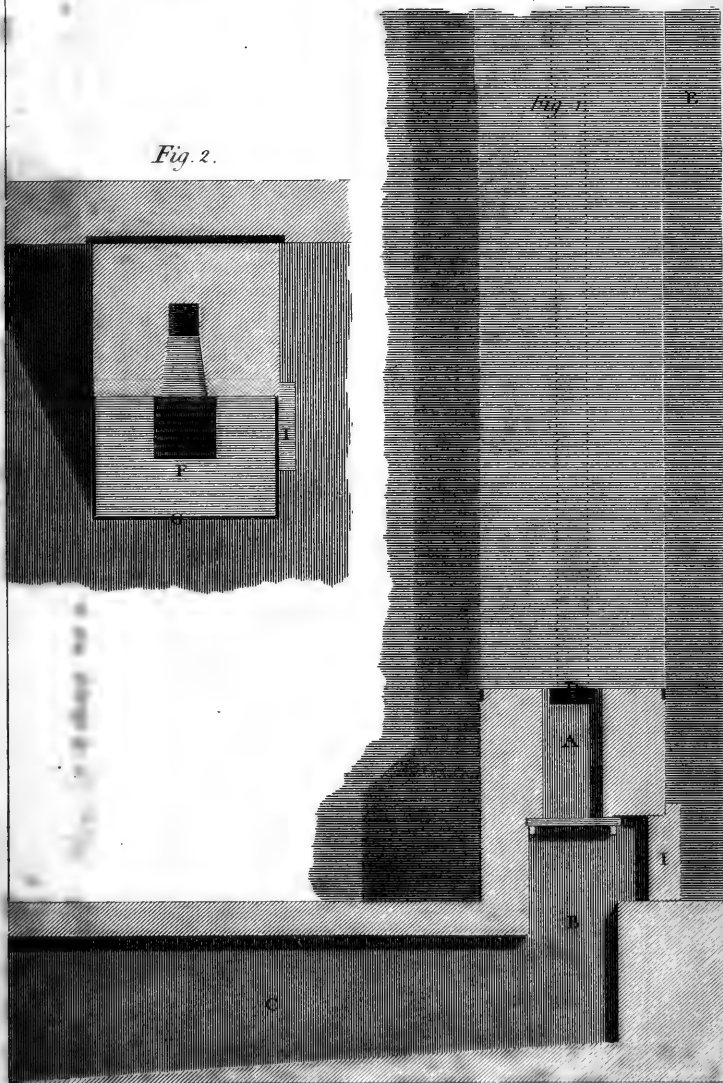




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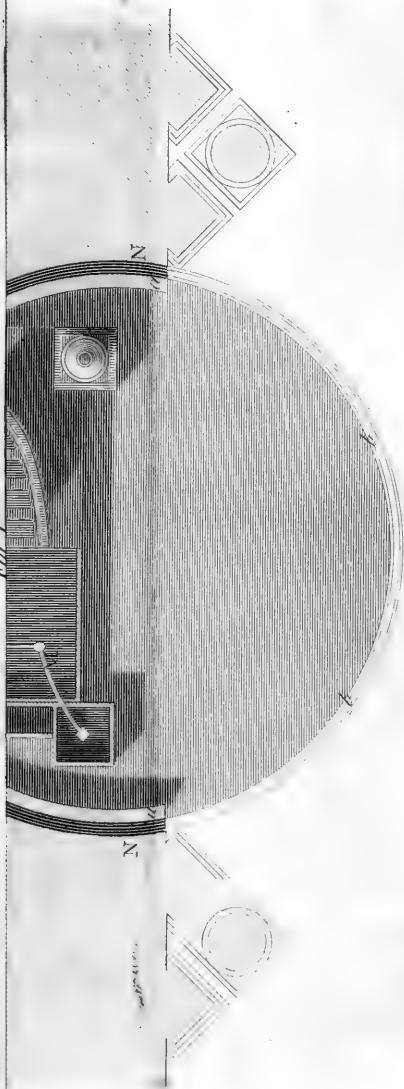


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*Fig. 1*



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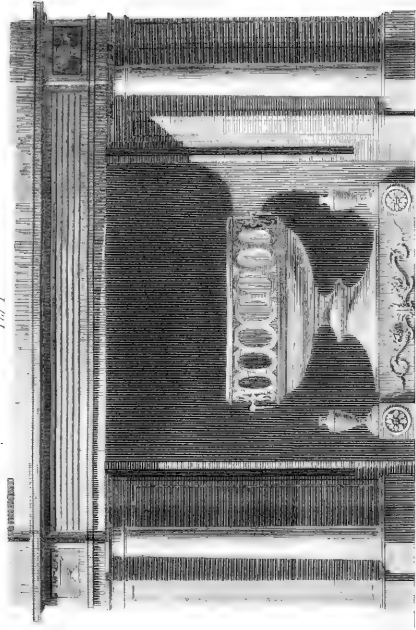
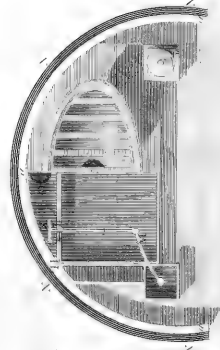


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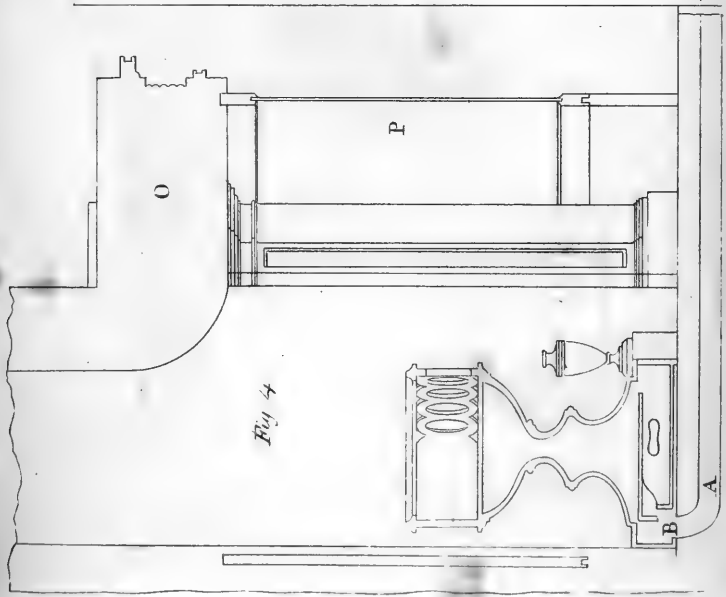
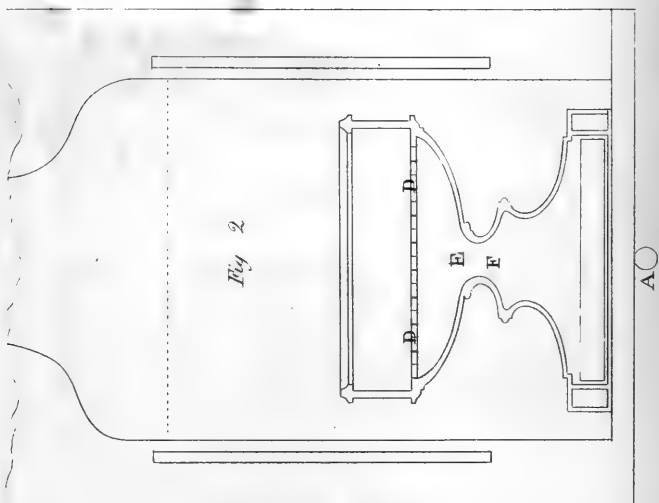






Fig. 3.



Fig. 6.

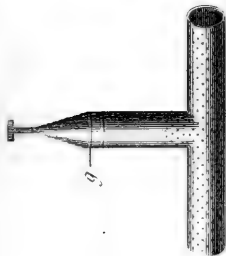


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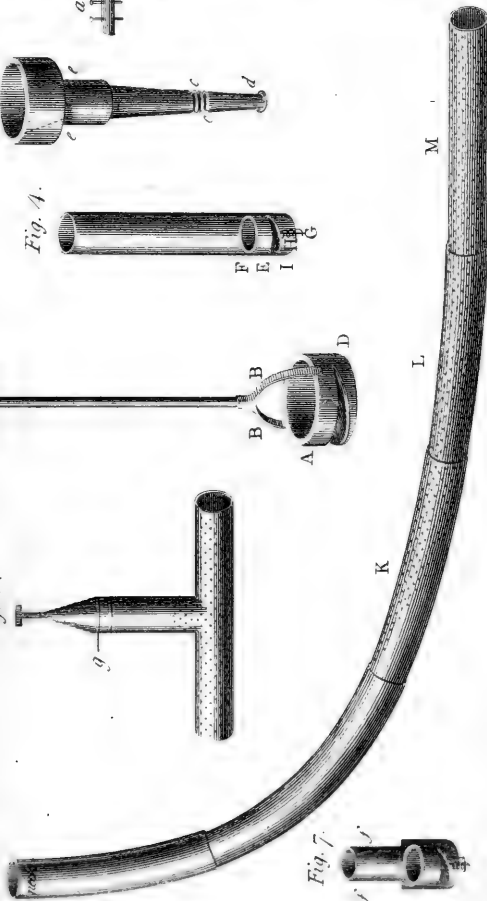


Fig. 2.



Fig. 4.



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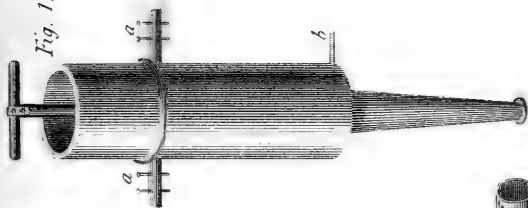


Fig. 7.





